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Abstract

We study the link between the student credit expansion of the past fifteen years and the contemporaneous rise in college tuition. To disentangle simultaneity issues, we analyze the effects of increases in federal student loan caps using detailed student-level financial data. We find a pass-through effect on tuition of changes in subsidized loan maximums of about 60 cents on the dollar, and smaller but positive effects for unsubsidized federal loans. The subsidized loan effect is most pronounced for more expensive degrees, those offered by private institutions, and for two-year or vocational programs.

Key words: student loans, college tuition

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1 Introduction

The existence of a causal link between student loan availability and college tuition has been the subject of policy discussion and debate for at least three decades (Bennett, 1987, for example), and has been no less relevant in recent years as tuition and student loan balances have continued to significantly outpace overall inflation. Average sticker-price tuition rose 46% in constant 2012 dollars between 2001 and 2012 (Figure 1), and despite a sharp deleveraging of other sources of debt by U.S. households after the Great Recession, student debt has continued to grow unabated, and now represents the largest form of non-mortgage liability for households (Figure 2). While rising tuition almost certainly contributes to increased demand for student loans, an important policy question is whether student loan supply may also allow tuition to rise as postulated by the so-called "Bennett Hypothesis."[1]

In this paper, we propose an identification strategy to isolate the effect of student loans on tuition. We use variation in student credit supply that resulted from legislative changes in the maximum amounts students are eligible to borrow from the federal subsidized and unsubsidized loan programs. These policy changes went into effect in the 2007-08 and 2008-09 school years and led to a large credit expansion, as these program maximums had remained unchanged since the early 1990s.[2] Exploiting the federal increase in credit supply for identification presents two challenges. First, the increase in program maximums affected students at all institutions. Second, we only have reliable time series data on the sticker-price of tuition rather than the net tuition paid by students after accounting for scholarships or discounts to lower-income students. In an illustrative model, however, we show that even when universities price-discriminate, a credit expansion will raise tuition paid by all students, not just students borrowing at the federal loan caps, because of pecuniary demand externalities. We also show that the tuition effects will be larger at schools

[1]The then-Secretary of Education William Bennett (1987) argued that "[...] increases in financial aid in recent years have enabled colleges and universities blithely to raise their tuitions, confident that Federal loan subsidies would help cushion the increase," a statement that came to be known as the "Bennett Hypothesis."

[2]The maximum subsidized federal loan amount for freshmen rose in the 2007-08 academic year from $2,625 to $3,500, and for sophomores from $3,500 to $4,500; unsubsidized loan maximums rose by $2,000 in the academic year 2008-09. Pell Grant maximums, which is not our main focus but that we control for, rose gradually between the 2007-2008 and 2010-2011 school years as well as in prior years as a result of the yearly appropriation process of the Department of Education. Subsidized, unsubsidized loans and Pell Grants are the main "Title IV" programs. We discuss the institutional details of federal aid programs in Section 3.

serving a more credit-constrained population. We use these predictions to identify the impact of loan supply increases on tuition by constructing an institution's "exposure," or treatment intensity, to a policy change using detailed student-level data. We then interact our exposure measure with the timing of shifts in the supply of federal student aid, with an approach similar to Card (1999)'s analysis of changes in national minimum wage standards.

We first validate our approach by documenting that institution-level loan amounts respond to the interaction of the legislated changes in maximum aid amounts with an institution's exposure to the changes. Changes in per-student subsidized (unsubsidized) loan amounts measured at the institution level load with a coefficient of .7 (.6) on yearly changes in the maximums per qualifying student. We next study the response of tuition to the interaction of policy changes and treatment intensity to examine tuition increases in the same year as the credit expansions. We find that increases in institution-specific subsidized (unsubsidized) loan maximums lead to a sticker-price increase of about 60 (40) cents on the dollar. This effect represents the additional amount that institutions raised their tuition in the years of the policy changes relative to what would have been expected without the policy change, which we measure using institutional fixed effects to capture the average tuition increases at an institution. All of these effects are highly significant and consistent with the Bennett Hypothesis, and apply to a large sample of all Title IV institutions. Direct quotes from earnings calls and large stock market reactions to the passing of these loan expansions lend additional support to these findings for the subset of publicly traded for-profit institutions.

The effect that we document is particularly interesting because it is evidence of a cross-demand effect of a credit expansion through a pecuniary externality with a relaxation of the borrowing constraint for some students affecting pricing to other students. Of course, institutions may have mitigated the effect of these increases through increases in institutional grants to some or all students. Though institutional grant data is not available for our entire sample, we find that an increase in subsidized loans actually decreased institutional grants by about 20 cents on the dollar (compared to an effect of about 30 cents for Pell Grants) for the subset of institutions that report grant amounts, suggesting that the tuition effect is on average not canceled out, and may even be amplified, by institutional grants, though we cannot observe the distribution of grants.

In robustness checks and alternative specifications, we attempt to address concerns that other variables may be driving the behavior we observe. First of all, our specifications control for changes in Pell Grant maximums, which partially overlapped with changes in the federal loan policies. Second, we address issues relating to the parallel trends assumption in a few ways. One obvious concern is that the Great Recession may have may have boosted demand for education services at institutions where students are more dependent on student aid, or on the supply side, these same institutions may have experienced a drop in state appropriations or endowments, requiring an increase in tuition to bolster budgets. However, tuition decisions for the year when the main policy took effect, academic year 2007-2008, predated the recession, as tuition is typically set in the first half of each calendar year. We provide estimates that drop the later years in our sample as a robustness check. Using the full sample, we also try to control for the differential characteristics of schools that may be driving differential variation in these years by interacting policy changes with other institution-characteristics such as changes in non-tuition funding sources, selectivity, cost, type of programs offered, or average student income. Our final robustness check is agnostic about what variables might be driving the differential variation and shows that the difference between this institution is indeed starkest in the policy years. We run placebo regressions that comparing tuition changes of highly and less exposed institutions outside the years of policy changes. We find that the subsidized loan effect is robust across specifications both in magnitude and significance, and passes the placebo test, but find that the unsubsidized loan effects are less robust to these controls and tests.

In addition, we investigate the characteristics of the institutions where the passthrough effect of credit to tuition are most pronounced. We find that the subsidized loan effect is most pronounced for more expensive degrees, those offered by private institutions, and for two-year or vocational programs. Finally, in a larger sample we focus on for-profit institutions, which, despite having received much attention in the policy debate, are heavily underrepresented in our main data sources. We document abnormally large tuition increases by this sector relative to other years and other sectors, providing suggestive evidence that for-profits institutions, which rely heavily on federal aid, were highly responsive to these credit expansions.

Related literature. This paper contributes to three main strands of literature. First, it builds on the expanding finance literature studying the role of credit supply on real allocations and prices. Much attention has been devoted to this question in the context of the housing market, for which credit is central, in an attempt to establish whether the U.S. housing boom of 2002-6 and the ensuing bust can be explained by increased credit to subprime borrowers (see, for example, Mian and Sufi, 2009; Adelino, Schoar, and Severino, 2012; Favara and Imbs, 2015). From a finance perspective, the market for postsecondary education has shared several features with the housing market despite the important difference that student loans fund a capital investment while mortgages fund an asset. Like housing finance, credit plays a key role in funding U.S. postsecondary education, and most of this credit is originated through government-sponsored programs. Our paper provides complementary evidence to the conjecture that credit expansions can result in aggregate pricing effects and not just on assets purchased by credit recipients.

This paper also contributes to the economics of education literature studying the determinants of the price of postsecondary education, and in particular, the strand of this literature that seeks to accept or reject the "Bennett Hypothesis." The literature on this topic has thus far not reached a consensus. The majority of these studies have focused on the effect of Pell Grants on sticker tuition[3], though other studies have used individual-level data to look for evidence that grant programs and tax credits may displace institutional grants that would otherwise lower the net tuition paid by aid recipients.[4] Our study is one of only a few to look at the impact of loan programs. Cellini and Goldin (2014) study the impact of overall federal aid eligibility by constructing a dataset of comparable eligible and ineligible for-profit institutions and show that eligible institutions charge tuition that is about 75 percent higher than comparable institutions whose students cannot apply for such

[3]For example, McPherson and Schapiro (1991), looking at the period 1979-1986, find no evidence of the Bennett hypothesis for private four-year institutions, but find a pass-through of $50 for every $100 for public four-year institutions. Singell and Stone (2007) find increases at private institutions but only in out-of-state tuition at public institutions using data from 1989 to 1996. Rizzo and Ehrenberg (2003) find evidence of the Bennett Hypothesis in in-state tuition, but not out-of-state tuition in a restricted sample of 91 public flagship state universities between 1979 and 1998.

[4]For example, Turner (2014) uses a regression discontinuity approach and finds that institutions alter institutional aid (scholarships) as a means of capturing the federal aid provided through the federal Pell Grant program. Similar studies have also found evidence of the Bennett Hypothesis in tax credits (Long (2004b), Turner (2014)), and state grant aid programs (Long (2004a)). A review of some of these and other studies of the Bennett Hypothesis can be found in Congressional Research Service (2014).

aid. Because almost all degree-granting institutions are federal-aid-eligible, their study is mostly limited to vocational programs. Our study looks instead at variation within eligible institutions (and thus includes two- and four-year degree programs), and also attempts to specifically isolate the role of student loans. The only studies to have explored this question specifically have used structural methods, e.g. Epple et al. (2013) and Gordon and Hedlund (2016). Both find that increases in borrowing limits generate tuition increases, with the latter finding that borrowing limit increases represent the single most important factor in explaining tuition increases between 1987 and 2010 at four-year institutions, explaining 40% of the tuition increase, while supply-side factors such as rising costs or falling state appropriations have much less explanatory power. Our study complements these studies by using a natural experiment approach.

Finally, this paper is related to the public economics literature on tax incidence (Kotlikoff and Summers, 1987), which studies how the burden of a particular tax is allocated among agents after accounting for partial and general equilibrium effects. In our setting, the student aid expansion is a disbursement of a public benefit. From an individual perspective, more aid is beneficial because of relaxed constraints, but in equilibrium the welfare effects of aid recipients could be negative because of the sizable and offsetting tuition effect.

The remainder of the paper is organized as follows. Section 2 presents the illustrative model, Section 3 provides institutional detail on federal aid programs and caps, and Section 4 introduces the data. Section 5 describes the empirical method. Section 6 discusses the main results in the paper, while Section 7 presents robustness specifications, studies attributes of institutions with the highest passthrough for the subsidized program and additional evidence for for-profits institutions. Finally, Section 8 concludes.

2 Model

We present an illustrative model to explain how increased student loan supply may affect sticker tuition, as well as the empirical identification assumption. A distinguishing feature of college pricing is the extent to which price discrimination takes place, with universities often using scholarships, grants, or other mechanisms to offer different prices to students of different incomes, skills, or backgrounds. Eligibility for most federal student aid, on the other hand, is based solely on income considerations. We consider a school that conditions tuition offers on students' observable characteristics. In the model, an increase in the federal student loan maximum boosts demand from lower-income students by relaxing their borrowing constraints. In equilibrium, the increased ability to pay raises tuition for all students, and not just for the aid recipients. This pecuniary demand externality is an important feature of the model, to explain how sticker price responds to changes in federal loans, although aid recipients are likely charged discounted prices rather than sticker. The tuition effect is also largest for universities in which a large number of students are exposed to the policy change, a result that we use in the empirical section to identify the effects of an increase in loan maximums on sticker tuition.

To simplify the exposition, we assume that short-run school capacity is fixed at N seats, so schools only decide whom to admit and what tuition to charge them. In reality short-run seat supply is imperfectly elastic rather than fixed, but only this more general assumption is needed for our main model predictions. Schools observe coarse measures of student characteristics along two dimensions: quality and income. A student i can be of high-quality, q_H, or low-quality, q_L, and either income-constrained, n_C, or unconstrained, n_U. A fraction of students s is constrained, and a fraction r is low-quality, and for simplicity the two characteristics are uncorrelated. We assume a population 1 of potential students and that student type is sufficiently large so schools can pick any type distribution, or $N < \min(s, 1-s, r, 1-r)$. Schools make tuition offers conditional on observables, meaning students at a school pay one of four tuition levels $t(q_i, n_i)$.

Students accept a school's tuition offer if their valuation of the school exceeds the tuition cost, and if they are able to afford the tuition cost given their income and aid. Thus, in addition to affecting the tuition they are charged, students' quality and income also determine their decision to attend. A student i's valuation of a school's offer depends negatively on her observed quality, because a high-quality student is likely to have better offers from other schools or employers. Additional unobserved components to both quality and income are present to capture residual uncertainty for a school as to whether a student accepts an offer and its ability to extract rent as in standard third-degree price discrimination models (Tirole, 1988). The idiosyncratic unobserved component to a student's valuation of a school's offer is distributed as $v_i \sim Exp(\delta)$, and she is willing to accept the school's offer when:

$$v_i - q_i \geq t(q_i, n_i) \tag{1}$$

Similarly, we assume that a student's unobservable income shock is distributed as $W_i \sim Exp(\omega)$. Constrained students are offered a federal student loan of balance B and thus can afford to attend if their income and aid are such that:[5]

$$W_i + n_i \geq t(q_i, n_C) - B, \tag{2}$$

An unconstrained student does not face a financial constraint and does not qualify for federal aid, i.e. W_U is sufficiently large that the financial constraint corresponding to (2) never binds. Because of the unobservable components, a school does not know with certainty whether a student accepts an offer. The demand from a high-income student with quality q_i is then equal to the probability that the student's unobserved valuation is sufficiently high:

$$d(q_i, n_U) = P(v_i \geq t + q_i) = e^{-\delta(t+q_i)} \tag{3}$$

while the demand from a low-income student with quality q_i is equal to the joint probability of a sufficiently high school valuation *and* income shock:

$$d(q_i, n_C) = P(v_i \geq t + q_i)P(W \geq t - B - n_C) = e^{-\delta(t+q_i)-\omega(t-B-n_C)} \tag{4}$$

where $t = t(q_i, n_i)$. The corresponding total demand functions from the four combinations of income and skills are given by the product of individual demands and the mass of students of each type combination.[6] Demand elasticities are δ for unconstrained students, and $\delta + \omega$ for constrained

[5] We are assuming that the interest charged is zero, as it is the case, for example for subsidized loan recipients when the student is in school. We are also assuming a fixed loan balance. In practice the loan balance is capped by the smaller of the loan maximum and the gap between cost of attendance and family contribution. We are therefore considering the case in which tuition levels are sufficiently high. This assumption can be relaxed.

[6] These are: $D^{H,U} = (1-s)(1-r)\, d(q_H, n_U)$; $D^{L,U} = (1-s)r\, d(q_L, n_U)$; $D^{H,C} = s(1-r)\, d(q_H, n_C)$; $D^{L,C} = sr\, d(q_L, n_C)$.

ones. Also let D^H, D^L, D^U, and D^C be the sums of the corresponding demand elements, or the aggregate demand from high-quality, low-quality, unconstrained, or constrained students, and D be the sum of all these terms.

We assume that colleges maximize a combination of student quality and revenues as in Epple, Romano, and Sieg (2006):[7]

$$\max_{t(q,n)} \gamma N^{-1}(q_H D^H + q_L D^L) + (1-\gamma)\Big(\sum_{(q,n)} t(q,n) D^{q,n} - cD\Big)$$

subject to:

$$D \leq N, \tag{5}$$

where γ is the weight placed by the school on the average quality of its student population, and $1-\gamma$ is the weight on profits. The school incurs a unit cost c to provide a seat up to its maximum capacity N. The equilibrium levels of t are obtained from the first order conditions of this objective function:

Proposition 1. *Let λ be the Lagrange multiplier on (5). Then the optimal tuition levels satisfy:*

$$\begin{aligned} t^{q,U} &= c + \frac{1}{\delta} - \frac{q\gamma}{(1-\gamma)} + \frac{\lambda}{1-\gamma}, \\ t^{q,C} &= c + \frac{1}{\delta+\omega} - \frac{q\gamma}{(1-\gamma)} + \frac{\lambda}{1-\gamma}. \end{aligned} \tag{6}$$

All proofs are provided in Appendix A. This proposition states that the tuition charged to each group of students is a markup over marginal cost c that is inversely related to their demand elasticity and to their quality. Thus, lower quality students pay higher markups, as do less constrained students who have lower demand elasticities.

To study how an increase in B may affect tuition, note that from (4) an increase in the borrowing cap leads to an upward parallel shift of the demand curve for given t. It follows, that increasing the borrowing amount B affects equilibrium tuition through the shadow cost of a seat and that the

[7] In Epple et al. (2006) schools maximize investment expenditure on students rather than revenues, but also balance annual budgets so that the two conditions are equivalent. See also Gordon and Hedlund (2016) for similar modeling assumptions.

effect is the same for all types of students:

Proposition 2. *An increase in the federal loan amount B leads to equal increases in $t^{H,U}, t^{L,U}, t^{H,C}$ and $t^{L,C}$:*

$$\frac{\partial t(q,n)}{\partial B} = \frac{1}{1-\gamma}\frac{\partial \lambda}{\partial B} = \frac{D^C \omega}{\delta N + D^C \omega} > 0 \tag{7}$$

for $q \in \{H, L\}, n \in \{U, C\}$.

The fact that the tuition effects are exactly equal relies on our specific assumption that all C students borrow the exact same amount, but the general prediction that there is a price effect across types from relaxing the constraint for the constrained type holds even when we relax this assumption.

In the empirical section, we study the response of tuition to an increase in federal student loan caps, which we model here as an increase in B. If loan maximums were the only factor influencing tuition, estimates of (7) could be backed out from average tuition increases in years when loan maximums were raised. However, since tuition trends are influenced by many other factors (e.g. the business cycle, changes in the returns to higher education, etc.), we abstract from these omitted variables using a difference-in-differences approach that exploits cross-sectional differences in the sensitivity of tuition changes to an increase in B. From (7), the effect of B on tuition is greater the more C students attend (D^C/N) and the higher the elasticity of C students versus U students ($(\delta + \omega)/\delta$). While elasticity differences are hard to measure, we use data on the share of aid recipients to measure D^C/N. However, because D^C/N is an equilibrium quantity, we show in the proposition below that the tuition effect is differentially larger for schools facing a higher s, i.e. the fraction of low income students in the population served by the school.

Proposition 3. *The larger the share of C students the higher the sensitivity of tuition to B.*

$$\frac{\partial}{\partial s}\frac{\partial t}{\partial B} = \frac{\delta N \omega}{(\delta N + D^C \omega)^2}\frac{\partial D^C}{\partial s} > 0. \tag{8}$$

The above proposition justifies our empirical approach of relating institutional exposures, calculated as the share of students who are constrained by a particular policy maximum, to predicted tuition increases in policy years. Given that our sample is composed of for-profit and not-for-profit institutions, a natural question is to what extent the tuition effect depends on γ. It turns out that the effect is ambiguous and depends on the difference between the quality of H and L students. This is because, γ and the distribution of student quality interact in determining the share of low-income students served by each institution.[8] In the empirical analysis, we study differential responses of tuition increases to shifts in loan caps as a function of D^C/N, and control for population quality and γ by including institution fixed effects in the empirical model.

3 Federal Student Aid Programs

Federal student aid programs are governed by Title IV of the 1965 Higher Education Act (HEA) and aim to support access to postsecondary education through the issuance of federal grants and loans.

Pell Grants are the main source of federal grants, and are awarded to low-income (undergraduate) students in financial need. Pell Grant disbursement averaged around \$30 billion in recent years, compared to an average of about \$70 billion for federal student loan originations to undergraduates (Figure 4).

The majority of federal student loans are administered under the William D. Ford Federal Direct Loan (DL) Program[9] and come in two types: subsidized and unsubsidized. The exact terms of federal loans have changed over time but typically involve low interest rates and flexible repayment plans. The federal government pays the interest on a subsidized student loan during in-school status, grace periods, and authorized deferment periods. Qualification for subsidized loans is based on financial need, while unsubsidized loans, where the student is responsible for interest payments, are not. Together, these two programs make up about 85% of federal student

[8]More precisely, we show in the appendix that

$$\frac{\partial}{\partial \gamma}\frac{\partial t}{\partial B} < 0 \Leftrightarrow \frac{D^{H,C}}{D^C} < \frac{\delta D^{H,U} + (\delta+\omega)D^{H,C}}{\delta D^U + (\delta+\omega)D^C} \tag{9}$$

[9]Historically, these were also administered under the FFEL program and known as "Stafford loans." Under FFEL, private lenders would originate loans to students that were then funded by private investors and guaranteed by the federal government. Under the DL program, the ED directly originates loans to students, which are funded by Treasury. With the Health Care and Education Reconciliation Act of 2010 the FFEL program was eliminated, but the types of loans offered to students were not affected.

loan originations, with the rest coming from PLUS and Perkins loans.[10] Federal loans are the principal form of student loans in the U.S., representing an even large share since the financial crisis (Figure 3).[11]

Eligibility. Federal student aid amounts are determined by *individual maximums*, which depend on the particular education cost and family income of a student, and by overall *program maximums* that apply to all students, which we use for identification.

Eligible students can qualify for federal loans and grants by filling out the Free Application for Federal Student Aid (FAFSA). The primary output from the FAFSA is the student expected family contribution (EFC), which represents the total educational costs that students and/or their families are expected to contribute, which is computed as a function of family and student income and savings, family size, and living expenses.

A student's aid package is determined through a hierarchical process starting with need-based aid, which includes Pell Grants and subsidized loans, as well as Federal Work Study and Federal Perkins Loans (which are small). Need-based aid is capped at a student's "financial need," or the portion of the cost of attendance (COA, the sum of tuition, room and board, and other costs or fees) that is not covered by the EFC:

$$\text{Pell Grants} + \text{Subsidized Loans} \leq \text{Financial Need} \equiv \text{COA} - \text{EFC}, \tag{10}$$

where the left-hand side omits, for simplicity, other (less-important) need based aid. Pell Grants are subject to an additional EFC restriction, where only students with an EFC below a certain threshold are eligible, with the maximum amount offered decreasing with EFC. This is in contrast to subsidized loans, for which maximum amounts do not depend on EFC aside from (10). The hierarchical aid assignment is such that students who are eligible for a Pell Grant will be offered it to cover their

[10] PLUS loans require that borrowers do not have adverse credit histories and are awarded to graduate students and parents of dependent undergraduate students. Finally, Perkins loans are made by specific participating institutions to students who have exceptional financial need.

[11] Federal loan programs do not require repayment when still in school, and do not require a credit record or cosigner. Interest rates have varied and been both fixed and floating. Rates on all federal loans to undergraduates currently stand at 4.29 percent. Loan repayment starts after a six-month grace period following school completion, and standard repayment plans are ten years. Payments can be stopped for deferments (back to school) or forbearance (hardship). Under "income based repayment" plans, borrowers can limit their loan payments to a fraction of their income.

financial need before any loan or other need-based aid.

Eligibility for non-need-based federal aid (which include Unsubsidized Loans and PLUS loans) is determined by computing the portion of the COA that is not covered by federal need-based aid or private aid (e.g. institutional grants):

$$\text{Unsubsidized Loans} + \text{PLUS Loans} \leq \text{COA} - \text{Need-Based Aid} - \text{Private Aid}. \tag{11}$$

Irrespective of the individual maximums, aid amounts are always capped by each program maximum. Unsubsidized borrowing can also occur in circumstances where a student's financial need is below the subsidized program maximum. Students can borrow up to their personal need in subsidized loans and then borrow unsubsidized loans in an amount such that their joint subsidized and unsubsidized borrowing is equal to the subsidized program maximum.

Changes in program maximums. Table 1 shows the evolution of federal aid program maximums in our sample period. The subsidized maximum was raised in the 2007-2008 school year, unsubsidized loan maximums were raised in the 2008-2009 school year, and Pell Grant maximums were raised and frozen through a series of appropriations and acts. In this section, we discuss the policies that changed these maximums and their impact on aggregate student loan originations.

The Higher Education Reconciliation Act (HERA) of 2006 increased the yearly borrowing caps for subsidized loans, which had remained unchanged since 1992, for freshmen to \$3,500 from \$2,625 and to \$4,500 from \$3,500 for sophomores. Borrowing limits for upperclassmen remained unchanged at \$5,500. Signed into law in February of 2006, the act took effect July 1, 2007, so that the change was in place and well anticipated prior to the 2007-08 academic year. Though HERA impacted borrowing for subsidized loans and unsubsidized loans (because, as described above, the cap is technically a combined subsidized/unsubsidized borrowing cap), we expect this legislation to mainly increase originations of subsidized loans, since if eligible, students would always take out a subsidized over an unsubsidized loan. Thus, HERA would only affect unsubsidized borrowing for freshman and sophomores that met two criteria; first, they did not have enough financial need to qualify to take out the entire program maximum in subsidized loans, and second, they chose to borrow the difference between the program maximum and their personal maximum in the form of unsubsidized loans. These two joint conditions apply to less than one percent of students in our sample, suggesting that unsubsidized borrowing was not significantly increased in direct response to HERA. In comparison, roughly 22% of the freshman in our NPSAS sample borrowed subsidized loans up to the program cap in 2004.

The data confirm that HERA primarily impacted subsidized borrowing. In the 2007-08 year, subsidized loan originations to undergraduates jumped from $16.8 billion to $20.4 billion (Figure 3), and consistent with the higher usage intensity, the average size of a subsidized loan rose from under $3,300 to $3,700, as shown in Figure 5, which reports average loan amounts per borrower. Unsubsidized loan originations show much smaller increases in 2007-08, with the total amount borrowed by undergraduates increasing from $13.6 to $14.7 billion, and the average per-borrower amount increasing from $3,660 to $3,770. Because the majority of the impact of HERA was on subsidized borrowing, we subsequently refer to HERA as affecting the subsidized borrowing maximum to avoid confusion with legislation passed in subsequent years that primarily impacted unsubsidized borrowing.

We provide additional evidence that these increases were due to the changes in the program maximums using loan-level data from the New York Fed/Equifax Consumer Credit Panel.[12] This data cannot distinguish between federal and private student loans, or subsidized and unsubsidized loans, but in Figure 6, we produce a histogram of student loan amounts in the 2006-2007 school year and again for the 2007-2008 school year, after the policy change. The "before" plot shows a large mass of borrowers concentrated at the unconventional amount of $2,625, the subsidized maximum for freshmen borrowers. In contrast, the "after" plot shows the largest mass of borrowers concentrated at $3,500, the new maximum. The plots also show a large mass of borrowers at cap amounts established for upperclassmen before and after the policy change. This shift is evidence that there was a large and immediate effect of the policy change on loan amounts.

The second loan policy change we study is the Ensuring Continued Access to Student Loans Act of 2008. Prior to this act, in addition to the subsidized amounts discussed above, independent

[12] A number of papers have used this data to study loan repayments (see, for example, Lee, Van der Klaauw, Haughwout, Brown, and Scally, 2014). We use this alternative source because NPSAS data is only available in the years 2004, 2008, and 2012, and is a repeated cross-section rather than a panel.

students were eligible for as much as $5,000 ($4,000 for freshman and sophomores) in additional unsubsidized loans. Dependent students were ineligible for these additional loans.[13] This act increased the maximums by $2,000 for all students, meaning dependent students were eligible for $2,000. Figure 3 shows that undergraduate unsubsidized loan originations jumped from under $15 billion to $26 billion in one year. It is worth noting that the act was passed in anticipation of private student loans becoming more difficult to obtain due to the financial crisis, and so some or all of these new originations may have partly replaced private loans. Additionally, the act was passed in May of 2008, after many financial aid packages had already been sent out for the academic year 2008-2009. Schools were told they could revise their offers to accommodate the new policies for the upcoming school year, which seems to have been often the case based on the data series. That said, due to the timing of the change, the full impact of the higher caps may have had real effects in more than a single year.

While Pell Grants are not the main focus of this paper, Pell Grant maximums were adjusted several times during our sample period, and are therefore included in our analysis. Maximums rose gradually from $3,375 to $4,050 between 2001 and 2004 through the appropriation process. They were then frozen at $4,050 for four years, until the Revised Continuing Appropriations Resolution of 2007 increased the maximum Pell Grant to $4,310 for the 2007-2008 school year, and the College Cost Reduction and Access Act, passed by Congress on September 7, 2007 scheduled more increases from $4,310 in 2007-2008 to $5,400 by the 2010-2011 school year. These maximums are only available to students with an EFC below a certain threshold. However, students with slightly higher EFCs are eligible for smaller Pell Grants, according to a scale. For all of the policy changes we consider, these smaller Pell Grants increased proportionately with the maximum Pell Grant. Pell Grant disbursements are plotted in Figure 4 against aggregate loan amounts; both show large increases over our sample period.

Before turning to a systematic analysis of the effect of these policies on tuition, we provide some direct evidence of the relevance of these policy changes to tuition at for-profit universities

[13]Students must meet certain requirements (e.g. being over 24 years of age, being a graduate or professional student, or being married) to be considered an independent student by the Federal Student Aid office; otherwise, they are considered dependent and assumed to have parental support, and thus may qualify for less aid.

by looking at earnings call discussions between senior management at for-profit universities and analysts around the time of the policy changes we study. Below, we quote from an earnings call of one of the most prominent for-profit education companies, the Apollo Education Group (which operates the University of Phoenix) in early 2007:

> **<Operator>:** Your next question comes from the line of Jeff Silber with BMO Capital Markets.
> **<Q - Jeffrey Silber>:** Close, it is Jeff Silber. I had a question about the increase in pricing at Axia; I'm just curious why 10%, why not 5, and why not 15, what kind of market research went into that? And also if you can give us a little bit more color potentially on some of the pricing changes we may see over the next few months in some of the other programs?
> **<A - Brian Mueller>:** The rationale for the price increase at Axia had to do with Title IV loan limit increases. We raised it to a level we thought was acceptable in the short run knowing that we want to leave some room for modest 2 to 3% increases in the next number of years. And so, it definitely was done under the guise of what the student can afford to borrow. In terms of what we will do going forward with regards to national pricing we're keeping that pretty close to the vest. We will implement changes over time and we will kind of alert you to them as we do it.
> **Source:** Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP.

As evidenced by this quote, Title IV loan limit increases appear to directly affect how this institution chose to set its tuition in those years, and we provide additional excerpts in Appendix C. In Appendix D, we also show that the passage of the three pieces of student aid legislation were associated with nearly 10% abnormal returns for the portfolio of all publicly traded for-profit institutions. This is consistent with the fact that changes in Title IV maximums had large implications in terms of demand at these institutions. We turn to this issue in the rest of the paper using a statistical model.

4 Data

We overview the data sources and sample used in the analysis and provide a more detailed description of each of the data sources in Appendix E. We use data from three main sources from the Department of Education: Integrated Postsecondary Education Data System (IPEDS), Title IV Administrative Data from the Federal Student Aid Office, which we refer to as "Title IV" data, and the restricted-use student-level National Postsecondary Student Aid Survey (NPSAS) dataset.

Our measures of sticker price and enrollment come from IPEDS. IPEDS is a system of surveys conducted annually by the National Center for Education Statistics (NCES) with the purpose of describing and analyzing trends in postsecondary education in the United States. All Title IV institutions are required to complete the IPEDS surveys. Though IPEDS began in 1980, the survey covering sticker-price tuition was changed significantly in the 2000-2001 school year, and we thus start our sample in this year.

We measure federal aid amounts at the institution level using the Title IV Program Volume Reports, which report yearly institutional-level total dollar amounts and the number of recipients for each federal loan and grant program. These data are available beginning with the 1999-2000 academic year separately for subsidized loans, unsubsidized loans, and Pell Grants.[14] We end our sample in 2011-2012 to exclude the 2012-2013 school year and following years, when graduate students became ineligible to receive subsidized loans as a result of the Budget Control Act of 2011, which would complicate our measure of these loans.

Merging Title IV and IPEDS data, we obtain an annual panel of federal loan borrowing, Pell Grants, enrollment and sticker-price tuition for the universe of Title IV institutions. This sample contains 5,560 unique institutions. Institutional grant measures (graduate and undergraduate) are available from the IPEDS Finance survey for 60% of our sample.

Finally, we supplement the IPEDS/Title IV panel with NPSAS, a restricted-use student-level dataset from NCES. The primary purpose of the NPSAS data is to study student financing of higher education and they thus have detailed information on the amount and type of loans that each student takes out. NPSAS surveys have been conducted approximately every four years starting in 1988 with a nationally representative sample of about 100,000 students at a cross-section of Title IV institutions. We mainly rely on the 2004 NPSAS to document pre-policy cross-sectional variation that is only possible to observe with student-level data, since this data allows us to observe not just institutional-level loan and grant totals, but the number of students who are constrained by each of the policy maximums. The 2004 NPSAS contains this detailed financing data for students attending

[14]Unfortunately, it does not separate loans given to undergraduates and loans given to graduate students until 2011 (Pell Grants are only given to undergraduates). However, because imputing the amount for undergraduates would require making several assumptions, we measure loan and grant usage at an institution using the total dollar amount scaled by the enrollment count (undergraduate and graduate, on a full-time-equivalent (FTE) basis) of the institution.

1,334 unique institutions, with an average (median) of 104 (85) students surveyed per institution.[15] Our final estimation sample is dictated by the merge of the Title IV/IPEDS data with NPSAS. Depending on the specification, the number of institutions in the merged Title IV/IPEDS/NPSAS sample ranges between 650, for specifications that require a measure of institutional grants, and 1,060, the number of institutions in our primary sticker tuition specification.

Table 2 reports summary statistics for the variables included in the regressions.

5 Empirical method

We present the difference-in-differences specification used to isolate the impact of the federal loan credit expansion on tuition. Our empirical approach is similar to Card (1999), who studies the effect of a change in national minimum wage standards using a cross-state treatment effect based on the fraction of workers earning less than the minimum wage before the policy. In our setting, we construct an institution-specific treatment intensity measure based on the fraction of students in each institution that are eligible and that participate in the programs. We first discuss the construction of the treatment intensity, or "policy exposures," and then describe the empirical specification.

Policy exposures. We use the student-level dataset NPSAS to define a narrow identification criterion of the pre-policy importance of different types of aid at each institution. Consider first the case of subsidized loans. If a student's individual maximum is below the program maximum, she cannot qualify for the program maximum and is thus unaffected by any changes to it. Additionally, some students may choose to borrow less than the amount they are eligible for, and will thus also be unaffected. We thus define an institution's "exposure" to the subsidized loan policy change as the fraction of undergraduate students who borrowed subsidized loans at the policy maximum in 2004, since this corresponds to approximately the fraction of students we would expect to be able and willing to take advantage of the policy change to borrow more subsidized loans.

We also evaluate the effect of the 2008-2009 increase of $2,000 in additional unsubsidized loans for all students. We separately calculate the exposures of dependent and independent students at

15 We also employ the 2008 NPSAS survey for robustness, which contains 1,697 unique institutions with an average (median) of 111 (87) students surveyed per institution.

each institution, and take the sum as the overall institution exposure. For independent students, we again take the fraction of students who were borrowing at the independent policy maximum in 2004. For dependent students, who were previously ineligible for unsubsidized loans and became eligible through the policy change, we construct a shadow participation rate since we cannot observe past participation. This measure is the subset of eligible students, or the fraction of dependent students at each institution, that borrowed the maximum amount of subsidized loans that they were eligible for, including students who were not eligible for any subsidized loans.[16] The intuition for this rule is that a student that could, but did not, borrow in the subsidized program will not borrow in the unsubsidized program, as it is more expensive to do so, and should therefore not be counted as a student constrained by the unsubsidized program cap. However, this measure is likely not to be as reliable as the one for subsidized loans, since it assumes that any dependent student borrowing the maximum amount of subsidized loans would also borrow the maximum amount of unsubsidized loans once eligible.

Finally, for Pell Grants, changes in the maximum Pell Grant amounts shift the supply of grants for all grant recipients. Thus, the Pell Grant exposure variable is calculated as the percent of students at a given institution awarded any positive Pell Grant amount as of 2004. As we will see below, because the policy shift applies to all amounts - -rather than just a certain threshold – Pell Grant exposure displays a fairly high degree of correlation with EFCs, which also may complicate identification.

Table 2 reports summary statistics for the exposure measures as of 2004. About 15% of all students that borrowed were at the subsidized loan cap in 2004 compared to 27% of students at the unsubsidized cap. In contrast, about 34% of students received a positive (not necessarily the maximum) amount of Pell Grants. The exposures also display significant variation, with a standard deviation of between 14% (subsidized loans) and 21% (unsubsidized loans). The table also reports summary statistics for the exposure variables computed from the 2008 NPSAS, for those institutions that reported both in the 2004 (baseline sample) and in 2008 survey. Average levels of Pell Grant and unsubsidized loan exposures are very similar in the two surveys, but the subsidized ex-

[16]As discussed in Section 3, because subsidized loans are need-based, while unsubsidized loans are not, it is possible to be eligible only for unsubsidized loans.

posure is significantly smaller, owing to the fact that the second NPSAS wave takes place after the increase in the subsidized loan maximum. Indeed, as the maximums are increased, the fraction of capped students should drop unless all students at the old maximum jump to the new maximums.

Empirical specification. We regress the date t yearly change in institution i characteristic Y_{it}

$$\Delta Y_{it} = \sum_{a} \beta_a \, \text{ExpFedAid}_{ai} \times \Delta\text{CapFedAid}_{at} + \gamma X_{it} + \delta_i + \phi_t + \epsilon_{it}, \tag{12}$$

on a set of controls, where i denotes an institution, t is a year and a indicates either subsidized loans, unsubsidized loans, or Pell Grants. In the main result, the dependent variable Y_{it} is changes in sticker tuition. We also use changes in aid amounts as the dependent variable to validate the treatment intensity, and in additional results, explore effects using changes in institutional grants and enrollments as the dependent variable.

The main coefficient of interest is β_a, which measures the sensitivity of tuition changes to changes in the program maximums for each aid type a. The specification accomplishes this by interacting the program cap change ($\Delta\text{CapFedAid}_{at}$) with the institutional-level treatment intensity measure described above (ExpFedAid_{ai}). We estimate all three β_a coefficients simultaneously to control for correlations in exposures, timing of the policy changes and substitution effects. Our regressions are specified in changes with institutional fixed effects δ_i because there is wide dispersion across our sample in tuition charged (ranging from a few hundred dollars to about $45,000), and tuition increases are often set as a percent of past tuition. Institutional fixed effects allow us to control for the correlation of tuition increases with past tuition levels and look for abnormally large increases at the institution level. We validate that this allows us to meet the parallel trends assumption using placebo tests in Section 7. We include year effects to control for economy-wide factors (e.g. increased demand for postsecondary education) that may have induced all institutions to increase their tuition more in some years than others. Finally, we control for a set of other controls X_{it} interacted with the policy changes as described in the results section.

An alternative coefficient of economic interest is the sensitivity of tuition to the equilibrium institutional-level aid amounts. To obtain these, we consider an IV regression, where the first stage uses equilibrium aid amounts as the dependent variable Y_{it} in (12) to construct an instrumented change in each institution's per-student federal aid, $\Delta\widehat{\text{FedAid}}$. The second stage then regresses the date t yearly change in each institution i variable of interest T_{it}

$$\Delta T_{it} = \sum_a \phi_a \Delta\widehat{\text{FedAid}}_{ait} + \gamma X_{it} + \delta_i + \phi_t + \epsilon_{it}, \tag{13}$$

on this instrument. As before, the regression includes institution and year fixed-effects and a set of additional controls X_{it}. In contrast to the OLS estimates above, which measure the sensitivity of tuition to relaxing the program maximums or caps, these IV estimates measure the sensitivity of tuition to equilibrium changes in aid amounts, which are determined by the change in the caps as well as the elasticity of aid demand. If there are high aid elasticities, we expect ϕ_a and β_a should be very similar in magnitude. As discussed in Section 4, we measure financial aid levels with error because, among other things, they include both undergraduate and graduate amounts. Thus we focus mostly on the reduced form coefficient β_a, which is also most immediately policy-relevant, as opposed to the IV estimates of ϕ_a in the results that follow.

6 Main empirical results

6.1 Sticker tuition and aid sensitivity to changes in program caps

Baseline specification. Table 3 presents our main results on aid and sticker tuition sensitivies to the policy changes, measured as the product of the yearly change in each program cap (only varies over time) and the treatment intensity based on the fraction of students at each institution that qualify for (and are likely to accept) the increased student aid amounts. Each regression is estimated between 2001-02 and 2011-12 and includes year and institution fixed effects, with standard errors clustered at the institution level to account for serial correlation of the error terms.

Columns 1-3 validate our treatment measure by regressing yearly changes in student aid levels on the product of treatment intensity and policy change. In columns 1 and 2, we find that yearly changes in subsidized loans load on the institutional-level change in the loan maximum with a coefficient of .7, while unsubsidized loans load with a coefficient of .57 on the unsubsidized maximum, suggesting that the demand elasticity for subsidized loans is quite high, and slightly lower for unsubsidized loans. Both coefficients are different from zero and one at conventional levels. In column 3, we find a coefficient for Pell Grants of 1.2, which is significantly different from zero at the 1% level but not different from one at conventional statistical levels, suggesting that an increase in Pell Grant availability results in a one-for-one increase in the equilibrium grant amount disbursed, i.e. that the demand elasticity for these grants is infinite, which is unsurprising.[17]

It is also interesting to look at substitution across aid types: in column 3, we also observe that the coefficients of Pell Grant usage on changes in unsubsidized and subsidized loan maximums are close to zero, implying that a greater availability of these other sources do not displace Pell Grants. On the other hand, in columns 1 and 2, the institution-level Pell Grant maximum change enters each loan regression with a negative and statistically significant sign, suggesting that a greater availability of Pell Grants displaces loan aid. This crowd-out effect may be the result of a lower demand or reduced eligibility for loans as implied by equations (11) and (10) and is consistent with Marx and Turner (2015) who find using a kink regression discontinuity design that increases in Pell Grant aid lower student loan borrowing.

Having documented the large responses of federal aid amounts to our treatment variables, we focus next on the response of sticker tuition to these treatments. Point estimates (column 4) suggest that a dollar increase in the subsidized cap and unsubsidized caps result in a 58 cent increase in sticker price (t-stat = 3.4), and 17 cent increase (t-stat = 4), respectively, and a dollar increase in the Pell Grant maximum (column 6) translates into a 37 cent increase in sticker price (t-stat = 2.5). The estimates provide support to the Bennett Hypothesis, with an average passthrough of increased student aid supply to tuition of around 40 cents on the dollar, although there is substantial heterogeneity across aid types. This is a large effect, and because it applies to sticker price tuition, it is likely affecting both the recipients of these loans as well as other students who do not borrow through the federal student loan program to fund their education. Although our focus and model is on student loans, one may have expected the largest tuition sensitivity to be on Pell Grants. While differences in passthroughs are not statistically significant, we note that changes in caps for Pell Grants took place over a number of years, which may attenuate the magnitude of the

[17] For brevity, the model in Section 2 abstracts from differences in interest and principal payment across types of aid. But a straightforward extension would predict that the elasticity of Pell Grant demand should be infinite given that grants are not subject to repayment.

point estimate.

IV specification. Thus far we have estimated the direct sensitivity of sticker tuition to changes in the "treatment" of increased aid maximums. Because some previously constrained students may not want to or be able to take advantage of the full cap increase, these changes do not necessarily translate one-for-one into actual aid taken (as shown in the first three columns of Table 3). To study how much tuition increases for each additional dollar of actual aid received we report in column 4 estimates for the second stage of the IV regression of tuition on aid amounts where each aid measure is instrumented by the institution-specific measure of change in aid maximums. Changes in sticker-price tuition have a coefficient of 89 cents on the dollar on the change in subsidized loan amounts (t-stat $= 2.5$). The unsubsidized loan effect is smaller (t-stat $= 2.5$) and the Pell Grant effect is estimated at 53 cents on the dollar (t-stat < 2.9). All of these estimates are similar to the direct sensitivities of sticker tuition to the measure of institution-specific aid maximums because the coefficients in the first stage are close to one.

6.2 Net tuition, institutional grants and enrollments

Net tuition and institutional grants Because many universities award institutional grants based on need or merit, not all students pay the sticker tuition price for their education, and because many of these grants are need-based, it is likely that many students who borrow in the federal student loan program may not be paying sticker price. However, as discussed in Section 2, when capacity is imperfectly elastic in the short run, aid to one group of students will create a pecuniary demand externality that could impact the prices paid by non-aid recipients as well. The results of our baseline regression show that non-recipients (in particular, students paying sticker-price tuition) do indeed see price increases following an increase in loan supply. It is possible that universities increased sticker-price tuition while simultaneously increasing institutional grants and so it was only sticker-price-paying students who were affected by these increases. We investigate this question in this section and find that institutional grants do not completely cancel out the effect of price increases for these students; in fact, they tend to decrease alongside tuition increases, meaning they may actually amplify the effect in our baseline regression.

We measure institutional grants using the IPEDS Finance Survey. Unfortunately, we note that this is only available for 60% of our sample. As shown in Table 4, an increase in subsidized loans is associated with a decline in institutional grants of about 20 cents on the dollar (t-stat = 1.7), unsubsidized loans have a coefficient not significantly different from zero, and an expansion in Pell Grants is associated with a reduction in institutional grants of 30 cents on the dollar (t-stat = 2). The results for Pell Grants are consistent with Turner (2014), who, using a regression discontinuity approach, finds that institutions alter institutional aid to capture increases in Pell Grants. In column 2 we regress the difference in sticker price and institutional grants and find a sensitivity with respect to subsidized loans of about .88 (t-stat = 3), to unsubsidized loans of about .15 (t-stat = 2.2), and to Pell Grants of about .4 (t-stat = 1.6). Although only available for a subsample of the original sample, these results suggest that the increase in federal aid resulted in increases in net tuition similar to those in sticker tuition because of (at times) significant declines in institutional grants.

Enrollments One of the main motivations for federal student aid is to relax participation constraints in postsecondary education, so understanding whether enrollment, in addition to price, responds to changes in loan supply is crucial to assessing the welfare impact of these policies. In fact, we show in Section 2 that imperfect supply elasticity guarantees that there will be price effects to some degree, but it is interesting to measure to what degree enrollment effects are also present. To evaluate enrollment effects we regress annual changes in enrollment on our measures of treatment intensity interacted with the timing of policy changes. As shown in column 3 of Table 4, we find a positive and statistically significant coefficient on institution-specific changes in caps for Pell Grants, but an insignificant coefficient on subsidized loan caps and a significant but tiny negative coefficient on unsubsidized loan caps. The point estimate on Pell Grants is economically significant – for example the 2010 increase in Pell amounts at the mean Pell exposure ($(5350 - 4731) \times .34 = 210$) would have implied a boost in enrollment of about 3.5% – and is also consistent with the literature on grants and college participation (see for example the review of Deming and Dynarski (2009)).[18] The relative ordering of these effects is consistent with economic

[18]They conclude that most studies of federal aid find that additional grant aid is associated with significant increases in attendance (e.g. Seftor and Turner (2002) for Pell Grants; Angrist (1993), Stanley (2003), Bound and Turner (2002) for GI Bills; Dynarski (2003) for Social Security student benefit program), though, for Pell Grants the evidence is mixed, as (Hansen (1983) and Kane (1995) find no significant increase in attendance following the introduction of Pell Grants).

priors, since, as previously noted, demand elasticities are largest for Pell Grants because the principal does not have to be repaid. In addition, these results suggest, as we assumed in Section 2, that the enrollment effects of increased loan supply may be small in the short-run.

7 Additional empirical results

We first discuss the robustness of the empirical findings from the previous section. We then attempt to identify the set of institutions for which the passthrough from aid to tuition was strongest and focus on for-profit institutions, which are under-represented in main sample.

7.1 Robustness of baseline specification

We attempt to address two potential concerns about the estimated effects of the student credit expansion on tuition. The first is measurement issues related to the Great Recession and other shocks to institution funding. A second main concern is that treated and control groups differ along important dimensions which more generally affect their tuition levels even in the absence of the changes in student aid maximums. We address this latter concern by studying the parallel trends assumption and by interacting policy changes with other institution-characteristics.

Excluding the Great Recession Policy changes for the loan programs went into effect in the 2007-08 (subsidized loan limit) and 2008-09 (additional unsubsidized loan limits) academic years. One may be concerned about the impact of the Great Recession on tuition in these years. On the demand side, a high unemployment rate may have boosted demand for education services at institutions with a student population that is more dependent on student aid. On the supply side, these same institutions may have experienced a drop in state appropriations or endowments. Both of these effects could have led to disproportionate tuition increases. However, it is important to note that tuition decisions each academic year are generally made in the first half of each calendar year. This means that the increase in the subsidized loan maximum predates the recession, as tuition for the 2007-2008 academic year would have been set in the spring of 2007. The unsubsidized loan policy comes into effect before the failure of Lehman Brothers, but after the start of the recession. In Table 5, we present estimates of the baseline tuition specification for tuition (repeated in column 1),

Many fewer studies look at federal loan aid; one exception is Dynarski (2002) who finds a very small effect on attendance and a larger effect on college choice.

but only including data up to the 2008-09 and 2007-08 academic years. We find that the subsidized loan effect is unaffected by the shorter samples, and that the unsubsidized loan effect is robust to excluding years beginning with the 2009-10 school year (excluding 2008-2009 would exclude the main policy change).

Additional controls Our second robustness check adds a set of controls X_{it} to the baseline specification (12). Anecdotal evidence suggests that for-profit universities (e.g. those quoted in Section 3) may be more likely to take advantage of credit expansions as opportunities to raise tuition. Persistent differences in tuition increases between for-profit and not-for-profit universities would be captured by the institutional fixed effects in our baseline specification, but to allow for differential responses in the years of the policy changes we include an interaction between for-profit status and each of the the three changes in the program caps: $\langle \Delta \text{PGCap}_t, \Delta \text{SLCap}_t, \Delta \text{USLCap}_t \rangle$. The inclusion of these controls does not appear to significantly change the point estimates on the measures of institution-specific program caps (column 1 in Table 6) relative to the baseline (column 4 in Table 3).

The second column of Table 6 applies the same logic above to several other dimensions of heterogeneity that may differentially affect tuition and aid: namely, the type of degree(s) offered, (Figure 8), how selective and expensive they are, and the average income of the students enrolled. For example, if community colleges offering 2-year degrees experienced a boost in demand, and consequently increased tuition amid the high unemployment levels experienced during the Great Recession, this could potentially bias our coefficients. Once again, to the extent that these institutional characteristics affected tuition across all years, their effect would be absorbed by the institutional fixed effects that we include in the regressions. We thus interact a 4-year program dummy, the admission rate, average EFC, and average level of tuition (all measured in 2004) with the changes in the program caps. As shown in column 2 of Table 6, which includes these additional 18 controls, while the coefficient on the subsidized loan cap is largely unaffected both in magnitude and its significance, those on Pell Grants and especially the one on unsubsidized loans drop in magnitude and become insignificant.

We note that for Pell Grants in particular, the controls above absorb much of the variation of our treatment intensity measure, and thus it is unsurprising that our estimated treatment effect decreases subtantially. In Table 7, we report the correlation between the intensity measures with EFC and tuition and find that EFC is highly correlated with the Pell Grant exposure but displays low to moderate levels of correlation with unsubsidized and subsidized loans. This is because the exposure to Pell Grants is based on the fraction of students receiving any positive grant amount (which is highly correlated with institution's mean student income levels) while loan exposures are only based on students at caps (which depend on a specific percentile of the income distribution).

The final column of Table 6 controls for changes in other sources of funding that could be affecting tuition. As discussed in detail in Appendix B, universities fund their operations both from tuition revenue, and from other sources such as government appropriations and other sources, including private donations. Much discussion has been devoted to this topic (see, for example, Congressional Research Service, 2014) particularly in the context of changes in state funding and private contributions. We thus supplement the specification of column 1 with the 2-year change in these sources of institution revenue (to account for possible delays between the time in which these other sources of funding are known to administrators) as controls.[19] We find that declines in state and private funding are associated with increases in tuition. In this specification the Pell Grant coefficient again loses significance, while the coefficient on subsidized loans is unaffected. The coefficient on unsubsidized loans is lower in magnitude but remains marginally significant.

Placebo analysis In the baseline model (equation 12), we identified tuition and aid sensitivities from the regression coefficients β_a, on each interaction measure of institution-level program maximum changes. To see if more and less exposed institutions experienced similar tuition and aid trends in the years when caps were not raised, we follow e.g. Autor (2003), and analyze how the β_as would have been estimated had we (as a placebo) analyzed cross-sectional differences in tuition and aid in years where no actual policy occurred. For each aid of type a we estimate the following:

$$\Delta Y_{it} = \sum_s \zeta_{as}\, \text{ExpFedAid}_{ai} \times \mathbb{1}(\text{year} = s) + \sum_{\alpha \neq a} \beta_\alpha \text{ExpFedAid}_{\alpha i} \times \Delta\text{CapFedAid}_{\alpha t} + \gamma X_{it} + \delta_i + \phi_t + \epsilon_{it}. \tag{14}$$

[19]This data is only available for a somewhat smaller sample (8,000 observations versus 10,500 in the baseline).

Here we control for the other aid types (α) that are not subject to a placebo by interacting them with the corresponding actual changes in program caps as in the baseline specification (12). For aid a, instead, we estimate a series of yearly cross-sectional regressions of changes in tuition and aid on their exposures to aid. The coefficients ξ_{as} identify, in each year, abnormal changes in the dependent variables relative to the omitted or baseline year. We set the baseline year to be 2006, which is when the first of three major legislative acts affecting program caps was passed.

For each type of aid, time series estimates for ξ_{as} are shown as the orange lines in the top, middle and bottom panels of Figure 7. We also plot 95% pointwise confidence intervals, and include gray bars indicating the actual changes in each program maximum weighted by the average cross-sectional exposures (measured on 2004 NPSAS) for each aid type. For comparability, scales are set equal across all charts. For subsidized loans, the loading on subsidized exposure ξ_{as} of subsidized loan amounts (panel a) and tuition (panel b) spike coincident to the changes in subsidized maximums (gray bar) and are both significant at the 5% levels. For sticker-price tuition we indeed observe the largest spike in 2007-08, but also observe higher sensitivity in 2006-07 and 2008-09, which may be consistent with some sluggish tuition adjustment or anticipatory effects from announcement to implementation of these policies.

For unsubsidized loans we observe a very similar pattern with respect to loan amounts (panel c) with spikes on the loadings on exposure that are coincident to the policy changes (2007-08 and especially 2008-09). Tuition's loading on unsubsidized exposure (panel d) displays higher than average levels in 2006-07 and 2008-09, but only the 2008-09 change is significant. While it may be initially surprising that we do not observe a 2007-08 tuition increase, we note that because that policy affected both subsidized and unsubsidized loans and we control for the actual change in subsidized loans in our placebo regression, we have made it much more difficult to find this effect than in our baseline regression.

The bottom two panels show parameter estimates for Pell Grants. Policy changes for Pell Grants are much more gradual and take place in multiple years. In those years, the cross-sectional expansion in Pell amounts are significantly related to the institution exposures (significant at 5%) in contrast to the changes in sticker prices, which are not statistically larger in those years. In sum, the placebo tests confirm that NPSAS exposures are valid sorting variables for aid amounts. In terms of tuition, we find that cross-sectional differences in tuition changes with respect to aid exposures are coincident for subsidized and (to a lesser extent) for unsubsidized loan amounts, but not for Pell Grants.

In Appendix F, we show additional robustness checks where we measure exposures from the 2008 NPSAS wave rather than the 2004 wave, and where we specify the dependent variables in logarithm changes rather than level changes. In sum, we find a robust passthrough of federal aid to tuition in the form of subsidized loans and a weaker effect of unsubsidized loans and Pell Grants. For unsubsidized loans in particular, this weakness may be due to limitations to our identification approach, since, as we have discussed in Section 5, the exposures are more difficult to measure, and the policy change coincided with the contraction in the private student loan market and the Great Recession. It is also quite possible that subsidized loans, which represent a more significant subsidy than unsubsidized loans and are awarded to less needy students than Pell Grants, are in fact more economically meaningful in tuition-setting decisions. We believe the results we present on subsidized loans are new to the literature. We find a sensitivity of changes in tuition to changes in subsidized loan amounts on the order of about 40-60 cents on the dollar, with estimates that are highly significant in essentially all of the specifications considered.

7.2 Attributes of tuition-increasing institutions and the for-profit sector

Attributes of tuition-increasing institutions Results presented thus far indicate that changes in the sticker price of tuition are, on average, sensitive to changes in the supply of subsidized loans, Pell Grants, and unsubsidized loans, with a particularly robust subsidized loan effect. In this section we dig deeper into these results to characterize the attributes of institutions that displayed the largest passthrough effects of aid on tuition. For each form of aid, we interact in Table 8 the measure of institution-level exposure with key cross-sectional characteristics: whether a program offers four-year degrees, whether the school is a private institution, the tuition level in 2004, and the average EFC of the student population in 2004.[20]

[20] As the model of Section 2 points out in the context of γ, these interaction effects can be complex and non-linear. Because here we are estimating linear models, estimates are only picking up average effects.

In terms of changes in subsidized loan caps (column 1), we find that private institutions and non-four-year institutions (community colleges or vocational institutions) have larger average sensitivities, as did those that charged higher tuition (all results significant at 1% level). In addition, institutions with students having lower EFCs also displayed a higher sensitivity although the difference is not significant at conventional levels (t-stat = 1.5). Results for changes in the unsubsidized loan caps (column 2) are similar but are weaker in magnitude – only the tuition level difference is significant at conventional levels. Finally none of the interactions between the institution characteristics and the institution-level measures of changes in Pell Grant maximums are significant, suggesting homogeneous effects among institutions in our sample.

In conclusion, we find that expensive, private, or sub-four-year programs are associated with larger tuition responses to loan maximum changes, while responses to Pell Grants displayed a more uniform response across institutions.

Evidence for the for-profit sector Since the 1972 HEA re-authorization allowed for-profit institutions to be eligible to receive federal student aid, the market share of for-profit institutions has grown substantially (Deming, Goldin, and Katz, 2012). For-profit institutions now receive over 76.7% of their revenue, on average, through Title IV programs. This heavy dependence on federal aid has led to increased regulation and concern. Our data contains limited information on these institutions (less than 10% of institutions in NPSAS04). We presented some anecdotal evidence that for-profit institutions react to federal aid changes using earnings call discussions and stock market responses in Section 5. In Table 9, we provide additional evidence on the differential effect of these increases on for-profit institutions by comparing changes in aid amounts at for-profit (top panel) and other institutions (bottom panel) in our sample period. For each type of institution (and panel) we regress yearly changes on year dummy variables (reported at the top of each panel and with the year 2006, which is the year preceding the policy changes, serving as the omitted year) as well as on a policy year dummy variable which is equal to one for the 2007-08, 2008-09 and 2009-10 academic years when the federal aid changes went into effect (reported at the bottom of each panel). As shown in the bottom section of the panels, for-profit institutions experienced significantly larger increases in disbursed aid over the years of the aid cap changes. Correspondingly, these institutions also displayed sticker tuition increases of about $212, on average, as compared to $56 for not-for-profit institutions. These larger tuition increases are consistent with the results in the paper and the heavy reliance of for-profit institutions on federal student aid. This raw comparison has obvious limitations; for example, it does not allow us to control for other events specific to the for-profit sector that may have affected tuition. However, given the recent policy initiatives directly targeting aid for students attending for-profit institutions, a better understanding of the role of student borrowing for these institutions remains an open and important issue.

8 Concluding remarks

We studied the effects of a student credit expansion on tuition costs using a difference-in-differences approach around changes in federal loan program maximums to undergraduate students in the academic years 2007-08 and 2008-09. Consistent with the prediction of the illustrative model, institutions that were most exposed to these program maximums ahead of the policy changes experienced disproportionate tuition increases. We estimate tuition effects of changes in institution-specific program maximums of about 60 cents on the dollar for subsidized loans and 15 cents on the dollar for unsubsidized loans. The subsidized loan effects are robust to placebo tests and the inclusion of a large number of additional controls. Consistent with the model, we find that even when universities price-discriminate, a credit expansion will raise tuition paid by all students and not only by those at the federal loan caps because of pecuniary demand externalities. Such pricing externalities are often conjectured in the context of the effects of expanded subprime borrowing on housing prices leading up to the financial crisis, and our study can be seen as complementary evidence in the student loan market.

It is also important to note that while tuition increased steadily over our full sample period, the policy changes we exploit were concentrated in a few years later in the sample. This does not rule out a role of student credit in the observed tuition trends that is independent of policy changes. Previous work, for example, shows that greater aid availability tends to raise tuition levels more generally (Cellini and Goldin, 2014). In unreported results, we analyze pre-policy trends in aid and tuition. We find a positive association between ex-ante aid dependence (as of 2001-02), and subsequent aid growth in the pre-policy period. In terms of tuition, we observe a positive association with loan dependence for low initial aid levels consistent with binding credit constraints.

So do federal student loan programs represent bad policy? Importantly we also find a positive association between aid-dependence and subsequent enrollment expansion. This is in contrast to limited enrollment effects around changes in program maximums, likely owing to the differential elasticity of supply in the short- and medium-run. Expansion in enrollment means increased access to post-secondary education, which is particularly important given the positive gap between the cost of education and its social or private benefit.[21] Judging the welfare implications of federal student loan programs ultimately needs to weigh tuition effects and increased participation.

[21] While the literature disagrees on the exact magnitude of the returns to higher education (Card, 1999; Avery and Turner, 2012), the "college wage-premium" has been shown to be rising over the past two decades due to demand for skilled workers outpacing supply, and contributing to growing wage inequality in the US (Goldin and Katz, 2009). Given this premium, to the extent that greater access to credit increases access to postsecondary education, student aid programs may help lower wage inequality by boosting the supply of skilled workers.

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Figure 1: **Sticker Tuition and Per-student Federal Student Loans** This figure plots average undergraduate sticker-price tuition and average federal student loan amounts per full-time-equivalent student. Amounts shown are in 2012 dollars. Source: IPEDS/Title IV.

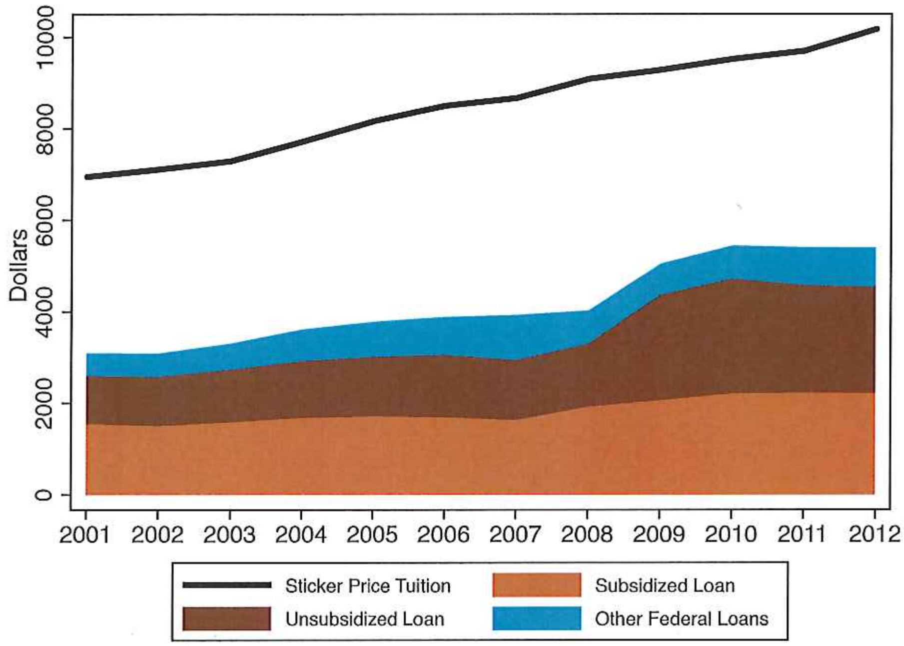


Figure 2: **Non-mortgage-related Household Debt Balances** This figure shows the time-series evolution of non-mortgage-related debt balances. Amounts shown are in nominal terms. Source: NY Fed CCP/Equifax.

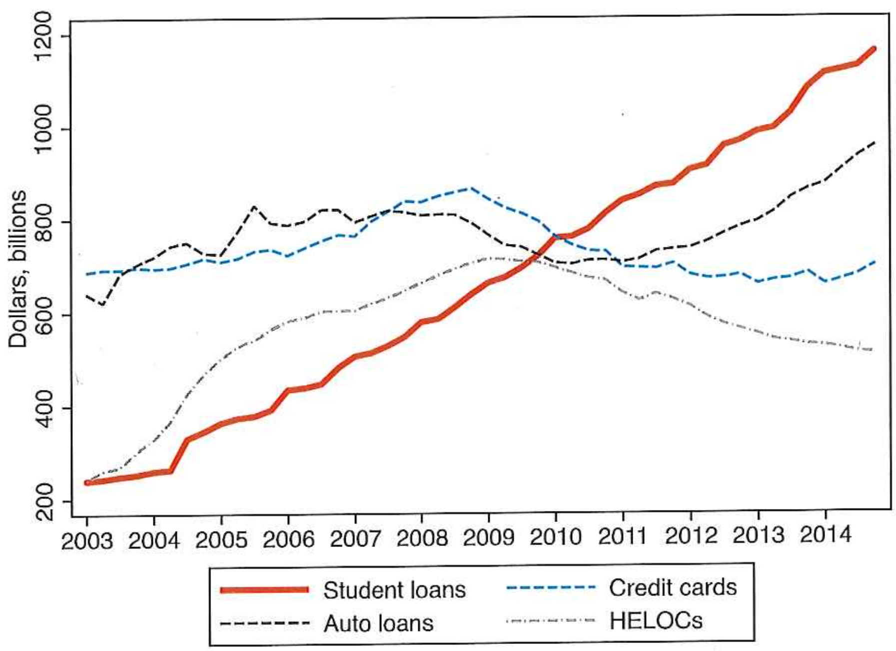


Figure 3: **Aggregate Student Loan Originations** This figure shows the time-series evolution of aggregate student loan originations by program type. Amounts shown are in nominal terms. Source: College Board.

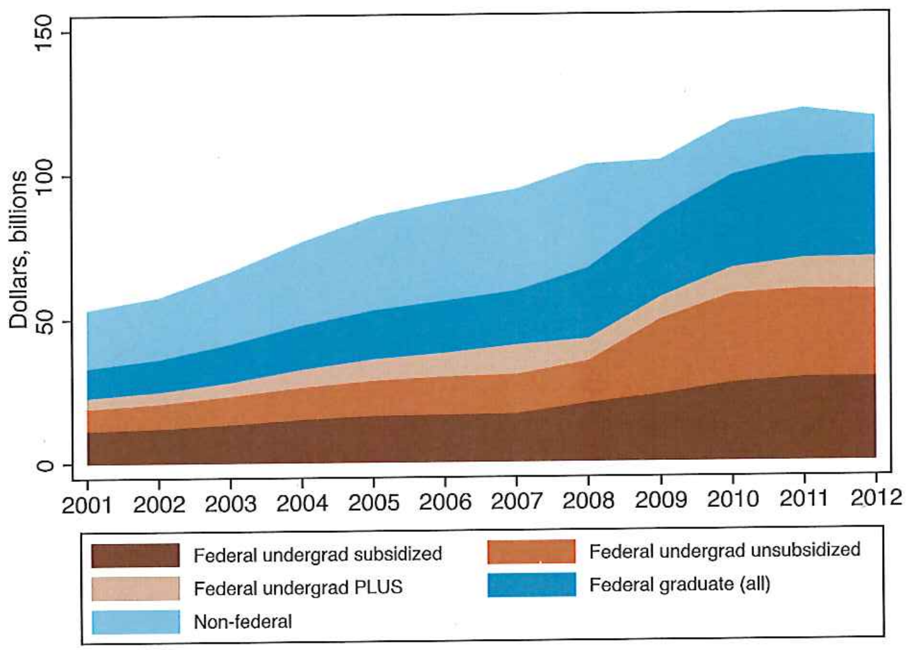


Figure 4: **Aggregate Pell Grant and Federal Loan Amounts** This figure plots Pell Grant disbursements by year as compared to total undergraduate federal student loan originations. Source: Title IV.

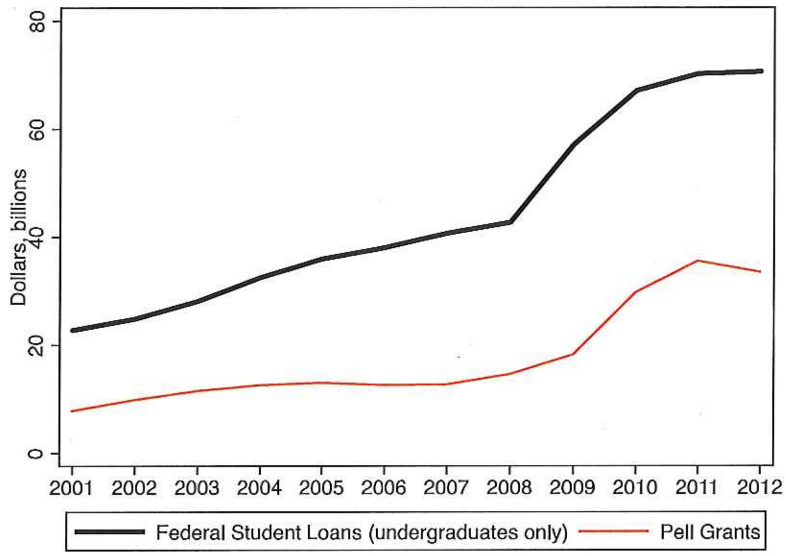


Figure 5: **Per-borrower Subsidized and Unsubsidized Federal Student Loan Amounts** This figure shows changes in the average borrowed amounts in the subsidized and unsubsidized loan programs. Source: IPEDS, Title IV.

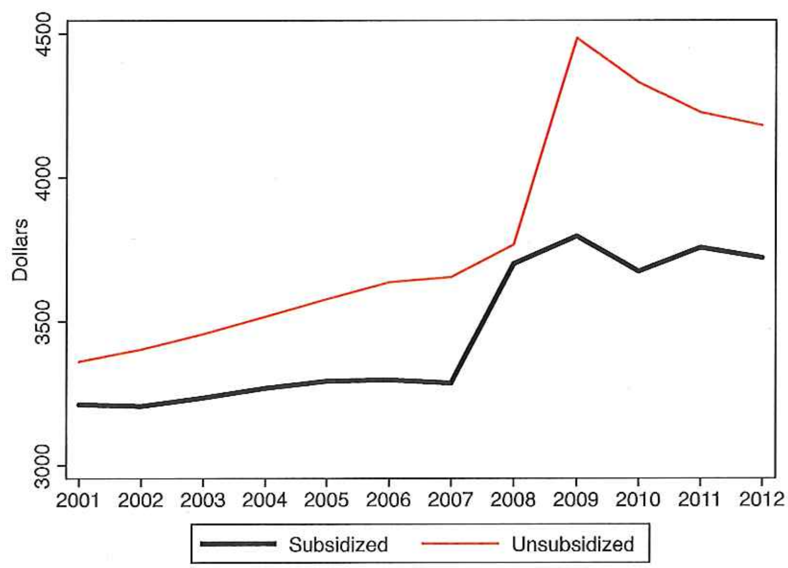
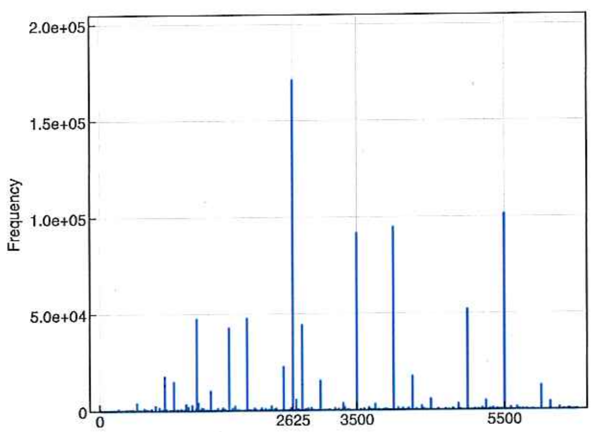


Figure 6: **Distribution of Student Loan Amounts** These figures plot the distribution of student loan amounts in the NY Fed CCP/Equifax panel in the year before (2006:Q3-2007:Q2) and after (2007:Q3-2008:Q2) the change in the subsidized loan maximum. The maximums are marked on the x-axis for each academic year. Source: NY Fed CCP/Equifax.

(a) **Student Loan Amount in 2006-2007**



(b) **Student Loan Amount in 2007-2008**

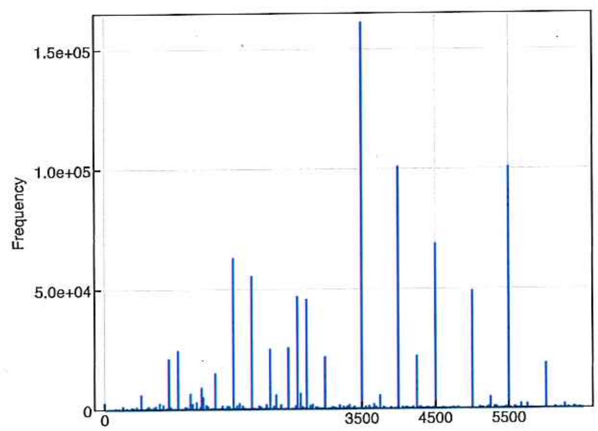
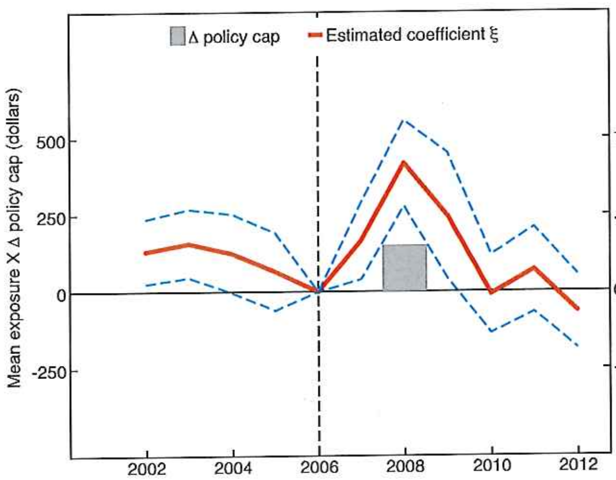
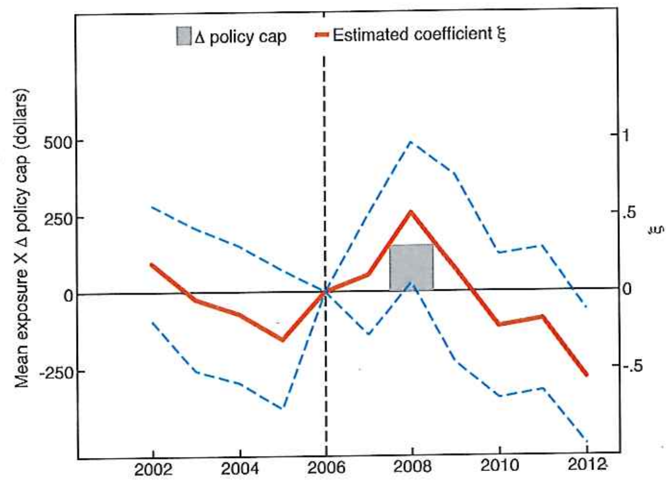


Figure 7: **Placebo tests** This figure shows a time series (orange) of estimated $\tilde{\xi}$ coefficients from equation (14) measuring the sensitivity of ΔAid and ΔTuition to an institution exposure to each type of aid. Vertical dotted black line (year 2006) is the baseline/omitted year in the regression. Dotted blue lines represent 95% confidence intervals. For each aid type, the gray bars show the actual mean change in program maximums, measured as the mean of yearly cap changes times institution exposures.

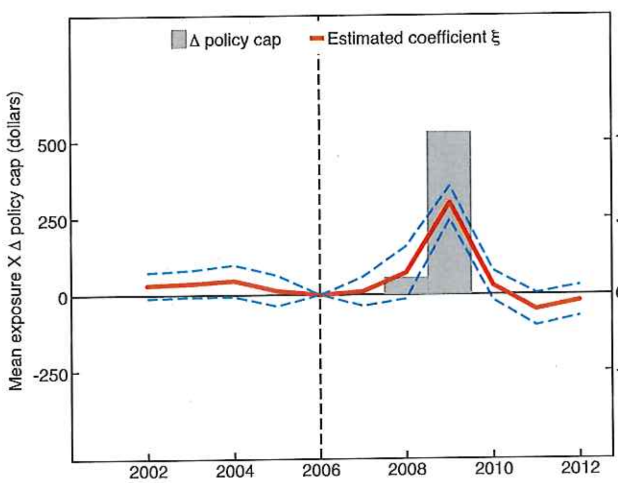
(a) **Subsidized loan exposure:** ΔSubsidized loans



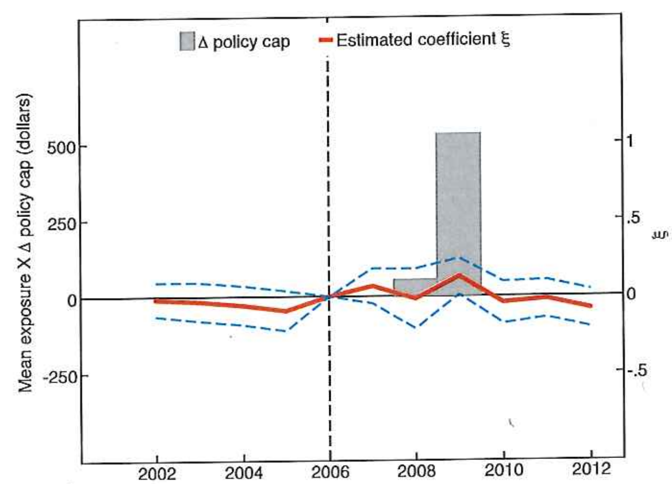
(b) **Subsidized loan exposure:** ΔTuition



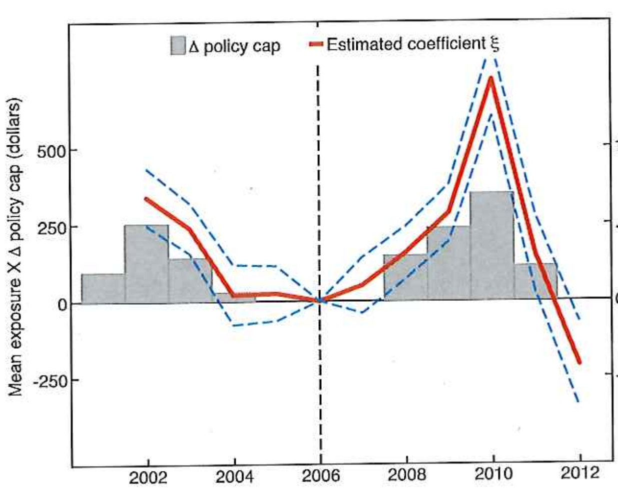
(c) **Unsubsidized loan exposure:** ΔUnsubsidized loans



(d) **Unsubsidized loan exposure:** ΔTuition



(e) **Pell Grant exposure:** ΔPell Grants



(f) **Pell Grant exposure:** ΔTuition

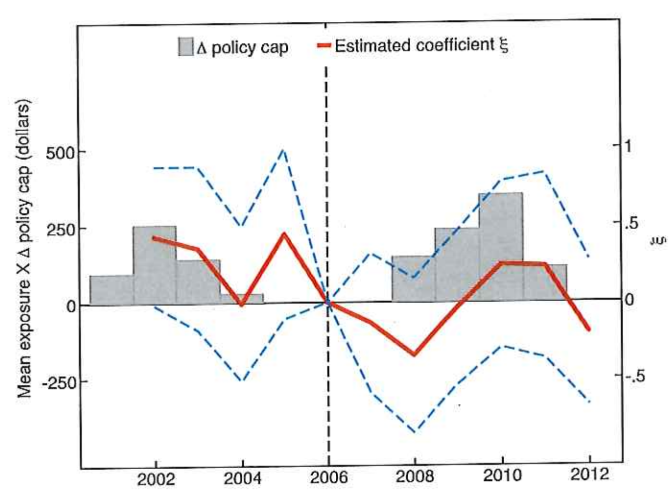


Table 1: **Changes in Title IV Federal Aid Program Maximums** This table shows changes to the maximums (caps) (reported as dollar amounts) of the Federal Direct Loan and Pell Grant Program. Y1, Y2, Y3, Y4, Grad are respectively the maximums for undergraduate freshmen, sophomores, juniors, seniors and graduate students. (D) and (I) refers to dependent and independent students. See Section 3 for more detail. Source: Higher Education Act, subsequent amendments and ED appropriations.

	Subsidized Loans				Unsubsidized Loans				Pell Grants
Year	Y1	Y2	Y3/Y4	Grad	Y1-Y4(D)	Y1/Y2(I)	Y3/Y4(I)	Grad	Y1-Y4
2000-01	2625	3500	5500	8500	0	4000	5000	10000	3350
2001-02	2625	3500	5500	8500	0	4000	5000	10000	3750
2002-03	2625	3500	5500	8500	0	4000	5000	10000	4000
2003-04	2625	3500	5500	8500	0	4000	5000	10000	4050
2004-05	2625	3500	5500	8500	0	4000	5000	10000	4050
2005-06	2625	3500	5500	8500	0	4000	5000	10000	4050
2006-07	2625	3500	5500	8500	0	4000	5000	10000	4050
2007-08	3500	4500	5500	8500	0	4000	5000	12000	4310
2008-09	3500	4500	5500	8500	2000	6000	7000	12000	4731
2009-10	3500	4500	5500	8500	2000	6000	7000	12000	5350
2010-11	3500	4500	5500	8500	2000	6000	7000	12000	5550
2011-12	3500	4500	5500	8500	2000	6000	7000	12000	5550

Table 2: **Summary statistics** This table reports summary statistics for the variables included in the regression tables. The unit of observation is a year (t) and institution (i). The sample starts in 2002 and ends in 2012. The Δ operator indicates annual changes (between year t and $t-1$). Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Additional detail on the variables are available in Section 4 and Appendix E.

	Mean	**St.Dev.**	**Min**	**Max**	**Count**
$\Delta\text{StickerTuition}_{it}$	743.97	730.09	-2832.00	4256.00	10560
$\Delta\text{PellGrants}_{it}$	109.60	254.49	-1691.52	2144.92	10060
$\Delta\text{SubLoans}_{it}$	83.68	267.29	-1781.18	1908.91	9780
$\Delta\text{UnsubLoans}_{it}$	148.30	431.71	-3060.49	3410.32	9740
SubLoanExp_{i}	0.15	0.14	0.00	0.74	10560
UnsubLoanExp_{i}	0.27	0.21	0.00	1.00	10560
PellGrantExp_{i}	0.34	0.19	0.00	1.00	10560
SubLoanExp08_{i}	0.08	0.08	0.00	0.60	6640
$\text{UnsubLoanExp08}_{i}$	0.27	0.18	0.00	0.83	6640
$\text{PellGrantExp08}_{i}$	0.38	0.15	0.00	0.97	6640
$\Delta\text{InstGrant}_{it}$	268.56	451.99	-1672.54	2249.36	5570
$\Delta\text{StickerTuition}_{it} - \Delta\text{InstGrant}_{it}$	687.19	692.33	-3478.73	4892.03	5570
$100 \times \Delta\log(\text{FTE}_{it})$	2.30	9.26	-47.99	53.48	9630
$\Delta^2\text{StateFunding}_{it}$	11.68	1012.93	-4765.55	4795.79	9420
$\Delta^2\text{FederalFunding}_{it}$	93.46	588.62	-3147.55	3346.77	9340
$\Delta^2\text{OtherFunding}_{it}$	260.32	1384.37	-7436.50	8131.24	9340
$\Delta^2\text{PrivateFunding}_{it}$	79.62	4348.30	-25832.35	26098.26	9340

Table 3: **Baseline regression specification** The first four columns in this table report OLS regression estimates of yearly changes in Pell Grants and subsidized/unsubsidized loan amounts per full-time equivalent student, and sticker tuition on interactions between cross-sectional institution exposures and yearly changes in program caps. The last column reports IV regression estimates of the effect of changes in federal loans and grants on sticker price tuition. The dependent variable is the annual change in sticker price tuition at the institution level. Observed changes in federal grants and loans per enrolled student are instrumented by the products of the corresponding aid exposures and changes in program caps, as described in the text. The unit of observation is a year (t) and institution (i). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	(1) ΔSubLoans$_{it}$	(2) ΔUnsubLoans$_{it}$	(3) ΔPellGrants$_{it}$	(4) ΔStickerTuition$_{it}$	(5) ΔStickerTuition$_{it}$
SubLoanExp$_i$ × ΔSLCap$_t$	0.664***	0.146	0.056	0.587***	
	[0.12]	[0.15]	[0.07]	[0.17]	
UnsubLoanExp$_i$ × ΔUSLCap$_t$	0.041*	0.544***	-0.039***	0.168***	
	[0.02]	[0.05]	[0.01]	[0.04]	
PellGrantExp$_i$ × ΔPGCap$_t$	-0.389***	-0.482***	1.152***	0.373**	
	[0.08]	[0.12]	[0.09]	[0.15]	
ΔSubLoans$_{it}$					0.891**
					[0.35]
ΔUnsubLoans$_{it}$					0.243**
					[0.10]
ΔPellGrants$_{it}$					0.527***
					[0.18]
Estimator	OLS	OLS	OLS	OLS	IV
Inst&Year FE?	Yes	Yes	Yes	Yes	Yes
Adj R^2	.07	.21	.44	.38	.
N Obs	9790	9740	10060	10570	9320
N Inst	990	990	1040	1060	970

Table 4: **Regression estimates for institutional grants and enrollments** This table reports OLS regression estimates of yearly changes in institution grant expenditure per FTE, difference between sticker price and institution grant expenditure and percentage growth rate of FTE on interactions between cross-sectional institution exposures and yearly changes in program caps. The unit of observation is a year (t) and institution (i). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

	(1) $\Delta\text{InstGrant}_{it}$	(2) $\Delta\text{StickerTuition}_{it} - \Delta\text{InstGrant}_{it}$	(3) $100 \times \Delta\log(\text{FTE}_{it})$
$\text{SubLoanExp}_i \times \Delta\text{SLCap}_t$	-0.201*	0.886***	-0.004
	[0.12]	[0.31]	[0.00]
$\text{UnsubLoanExp}_i \times \Delta\text{USLCap}_t$	-0.037	0.154**	-0.002***
	[0.05]	[0.07]	[0.00]
$\text{PellGrantExp}_i \times \Delta\text{PGCap}_t$	-0.330**	0.423	0.017***
	[0.16]	[0.28]	[0.00]
Inst&Year FE?	Yes	Yes	Yes
Adj R^2	0.31	0.17	0.08
N Obs	6290	5580	11050
N Inst	690	650	1000

Table 5: **Subsamples for baseline tuition regression specification** This table reports OLS regression estimates of yearly changes in sticker tuition on interactions between cross-sectional institution exposures and yearly changes in program caps. The unit of observation is a year (t) and institution (i). Column 1 reproduces column 4 in Table 3 and is estimated between 2002 and 2012. The other two columns restrict the estimation sample as noted. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

$\Delta\text{StickerTuition}_{it}$	(1) Full sample	(2) Pre-2009	(3) Pre-2008
$\text{SubLoanExp}_i \times \Delta\text{SLCap}_t$	0.587***	0.479***	0.502***
	[0.17]	[0.18]	[0.18]
$\text{UnsubLoanExp}_i \times \Delta\text{USLCap}_t$	0.168***	0.165***	-0.132
	[0.04]	[0.05]	[0.31]
$\text{PellGrantExp}_i \times \Delta\text{PGCap}_t$	0.373**	0.396*	0.570**
	[0.15]	[0.21]	[0.24]
Inst&Year FE?	Yes	Yes	Yes
Adj R^2	0.38	0.40	0.38
N Obs	10570	7720	6730
N Inst	1060	1050	1050

Table 6: **Regression estimates with additional controls** This table reports OLS estimates of the baseline model (Table 3) with the inclusion of additional controls. The additional cross-sectional controls (for which coefficients are not reported) are each interacted with the three changes in program caps $\Delta\mathbf{Caps}_t = \langle \Delta\text{PGCap}_t, \Delta\text{SLCap}_t, \Delta\text{USLCap}_t \rangle$. Changes in other sources or funding are computed over a two year period (Δ^2). The unit of observation is a year (t) and institution (i). The sample starts in 2002 and ends in 2012. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

$\Delta\text{StickerTuition}_{it}$	(1)	(2)	(3)
$\text{SubLoanExp}_i \times \Delta\text{SLCap}_t$	0.584**	0.470**	0.452**
	[0.18]	[0.21]	[0.20]
$\text{UnsubLoanExp}_i \times \Delta\text{USLCap}_t$	0.165**	0.004	0.093**
	[0.04]	[0.06]	[0.05]
$\text{PellGrantExp}_i \times \Delta\text{PGCap}_t$	0.335**	0.176	0.119
	[0.16]	[0.24]	[0.17]
$\Delta^2\text{StateFunding}_{it}$			-0.049**
			[0.01]
$\Delta^2\text{FederalFunding}_{it}$			-0.005
			[0.01]
$\Delta^2\text{OtherFunding}_{it}$			0.001
			[0.01]
$\Delta^2\text{PrivateFunding}_{it}$			-0.004**
			[0.00]
Inst&Year FE?	Yes	Yes	Yes
$\text{ForProfit}_i \times \Delta\mathbf{Caps}_t$	Yes	Yes	Yes
$\text{Four-year}_i \times \Delta\mathbf{Caps}_t$	No	Yes	No
$\text{AdmitRate04}_i \times \Delta\mathbf{Caps}_t$	No	Yes	No
$\text{EFC04}_i \times \Delta\mathbf{Caps}_t$	No	Yes	No
$\text{Tuition04}_i \times \Delta\mathbf{Caps}_t$	No	Yes	No
Adj R^2	0.38	0.38	0.37
N Obs	10570	10480	8790
N Inst	1060	1040	950

Table 7: **Correlation among institution characteristics** This table reports a correlation matrix between institution level characteristics measured as of 2004. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	SubLoanExp_i	UnsubLoanExp_i	PellGrantExp_i	EFC_i	Tuition_i	AdmitRate_i
SubLoanExp_i	1					
UnsubLoanExp_i	0.782	1				
PellGrantExp_i	0.197	-0.0411	1			
EFC_i	-0.0445	0.218	-0.731	1		
Tuition_i	0.273	0.500	-0.395	0.660	1	
AdmitRate_i	-0.145	-0.322	0.255	-0.424	-0.591	1

Table 8: **Sensitivity of aid exposures to institution attributes** This table expands on the baseline results of Table 3 by allowing the coefficients to vary across these institution characteristics: a dummy for private institutions, a dummy for 4-year programs, the 2004 levels of tuition and average EFC (both in thousands). See notes to Table 3 for more details. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)
ΔStickerTuition$_{it}$			
SubLoanExp$_i$ × ΔSLCap$_t$	0.264	0.546**	0.603**
	[0.28]	[0.17]	[0.17]
UnsubLoanExp$_i$ × ΔUSLCap$_t$	0.170**	-0.173	0.185**
	[0.04]	[0.11]	[0.04]
PellGrantExp$_i$ × ΔPGCap$_t$	0.384**	0.506**	0.541*
	[0.15]	[0.16]	[0.32]
SubLoanExp$_i$ × ΔSLCap$_t$ × Private$_i$	0.304**		
	[0.11]		
SubLoanExp$_i$ × ΔSLCap$_t$ × FourYear$_i$	-0.321**		
	[0.11]		
SubLoanExp$_i$ × ΔSLCap$_t$ × Tuition04$_i$	0.022**		
	[0.01]		
SubLoanExp$_i$ × ΔSLCap$_t$ × EFC04$_i$	-0.015		
	[0.01]		
UnsubLoanExp$_i$ × ΔUSLCap$_t$ × Private$_i$		0.061	
		[0.04]	
UnsubLoanExp$_i$ × ΔUSLCap$_t$ × FourYear$_i$		-0.047	
		[0.04]	
UnsubLoanExp$_i$ × ΔUSLCap$_t$ × Tuition04$_i$		0.009**	
		[0.00]	
UnsubLoanExp$_i$ × ΔUSLCap$_t$ × EFC04$_i$		0.000	
		[0.00]	
PellGrantExp$_i$ × ΔPGCap$_t$ × Private$_i$			-0.040
			[0.13]
PellGrantExp$_i$ × ΔPGCap$_t$ × FourYear$_i$			0.041
			[0.14]
PellGrantExp$_i$ × ΔPGCap$_t$ × Tuition04$_i$			-0.010
			[0.01]
PellGrantExp$_i$ × ΔPGCap$_t$ × EFC04$_i$			-0.007
			[0.02]
Inst&Year FE?	Yes	Yes	Yes
Adj R^2	0.38	0.38	0.38
N Obs	10570	10570	10570
N Inst	1060	1060	1060

Table 9: **Years of Federal Loan, Pell Grant, and Tuition increases for For-Profit and Not-for-Profit institutions** These tables regress annual changes in federal subsidized and unsubsidized loans, Pell Grants, and sticker price tuition against year dummies. The omitted dummy is for the year 2006. The Year = 2008,09,10 is a dummy varaible corresponding to those years, which is when the federal aid cap changes take effect. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

For-Profits

	$\Delta PellGrants_{it}$		$\Delta SubLoans_{it}$		$\Delta UnsubLoans_{it}$		$\Delta StickerTuition_{it}$	
Year = 2002	178**	[14]	-74**	[19]	-246**	[29]	25	[49]
Year = 2003	110**	[13]	-64**	[17]	-194**	[27]	226**	[46]
Year = 2004	-28**	[12]	-84**	[17]	-210**	[26]	36	[25]
Year = 2005	-112**	[14]	-115**	[18]	-252**	[27]	86**	[25]
Year = 2007	-35**	[14]	-50**	[18]	-317**	[27]	83**	[25]
Year = 2008	89**	[14]	460**	[20]	-117**	[27]	205**	[27]
Year = 2009	252**	[14]	-53**	[18]	670**	[29]	269**	[29]
Year = 2010	728**	[17]	-264**	[18]	-485**	[27]	269**	[29]
Year = 2011	106**	[16]	-215**	[18]	-576**	[28]	88**	[28]
Year = 2012	-485**	[18]	-249**	[19]	-374**	[30]	-102**	[30]
Constant	85**	[8]	164**	[10]	371**	[15]	487**	[15]
	$\Delta PellGrants_{it}$		$\Delta SubLoans_{it}$		$\Delta UnsubLoans_{it}$		$\Delta StickerTuition_{it}$	
Year = 2008,09,10	386**	[8]	148**	[9]	272**	[13]	212**	[16]
Constant	50**	[2]	67**	[2]	126**	[4]	523**	[5]
Inst FE?	Yes		Yes		Yes		Yes	
N Obs	18750		16980		16760		16880	
N Inst	2050		1910		1900		2090	

Not-for-Profits

	$\Delta PellGrants_{it}$		$\Delta SubLoans_{it}$		$\Delta UnsubLoans_{it}$		$\Delta StickerTuition_{it}$	
Year = 2002	-106**	[7]	-260**	[9]	-513**	[12]	-164**	[12]
Year = 2003	-157**	[7]	-165**	[9]	-456**	[12]	-38**	[13]
Year = 2004	-229**	[7]	-174**	[9]	-477**	[12]	60**	[14]
Year = 2005	-252**	[7]	-201**	[9]	-483**	[12]	33**	[13]
Year = 2007	-262**	[7]	-257**	[9]	-588**	[12]	6	[12]
Year = 2008	-161**	[7]	-22**	[10]	-445**	[13]	46**	[12]
Year = 2009	-76**	[7]	-223**	[9]	10	[16]	79**	[12]
Year = 2010	294**	[9]	-186**	[9]	-452**	[14]	54**	[13]
Year = 2011	-32**	[8]	-237**	[10]	-688**	[14]	36**	[13]
Year = 2012	-315**	[8]	-241**	[9]	-560**	[13]	90**	[12]
Constant	260**	[5]	292**	[5]	630**	[7]	618**	[7]
	$\Delta PellGrants_{it}$		$\Delta SubLoans_{it}$		$\Delta UnsubLoans_{it}$		$\Delta StickerTuition_{it}$	
Year = 2008,09,10	159**	[4]	16**	[4]	94**	[7]	54**	[7]
Constant	118**	[1]	134**	[1]	241**	[2]	623**	[2]
Inst FE?	Yes		Yes		Yes		Yes	
N Obs	39420		38390		37830		37850	
N Inst	3550		3440		3420		3630	

Appendix

A Proof of model propositions

Proof of Proposition 1 Letting λ be the Lagrange multiplier on the capacity constraint, the first order conditions are:

$$-\gamma N^{-1} q \delta D^{q,U} + (1-\gamma) N^{-1} D^{q,U} - (1-\gamma)(t^{q,U} - c)\delta D^{q,U} - \lambda \delta D^{q,U} = 0 \tag{15}$$
$$-\gamma N^{-1} q (\delta+\omega) D^{q,C} + (1-\gamma) N^{-1} D^{q,C} - (1-\gamma)(t_{q,C} - c)(\delta+\omega) D^{q,C} - \lambda(\delta+\omega) D^{q,C} = 0 \tag{16}$$

for $q = q_H, q_L$, where we have used the observation that $\frac{\partial D^{q,U}}{\partial t^{q,U}} = \delta D^{q,U}$ and $\frac{\partial D^{q,C}}{\partial t^{q,C}} = (\delta+\omega) D^{q,C}$. When $\lambda > 0$ (i.e. the constraint binds) this gives us the solutions above.

Proof of Proposition 2 We first note that:

$$\frac{\partial t(q,n)}{\partial B} = \frac{1}{1-\gamma}\lambda_B \tag{17}$$

for $q = q_H, q_L$ and $n = n_U, n_C$. We can solve implicitly for λ_B by taking the derivative of the constraint $D^U + D^C = N$ with respect to B.

$$\frac{\partial D^U}{\partial B} + \frac{\partial D^C}{\partial B} = 0. \tag{18}$$

Notice that:

$$\frac{\partial D^U}{\partial B} = -\frac{\delta D^U}{1-\gamma}\frac{\partial \lambda}{\partial B} \tag{19}$$
$$\frac{\partial D^C}{\partial B} = -\frac{(\delta+\omega) D^C}{1-\gamma}\frac{\partial \lambda}{\partial B} + \omega D^C \tag{20}$$

This gives us:

$$\frac{\delta D^U + (\delta+\omega) D^C}{1-\gamma}\frac{\partial \lambda}{\partial G} = \omega D^C \tag{21}$$
$$\lambda_B = \frac{(1-\gamma) D^C \omega}{\delta D^U + (\delta+\omega) D^C} \tag{22}$$
$$\lambda_B = \frac{(1-\gamma) D^C \omega}{\delta N + D^C \omega} \tag{23}$$

Thus we have that:

$$\frac{\partial t(q,n)}{\partial B} = \frac{D^C \omega}{\delta N + D^C \omega} > 0 \tag{24}$$

Proof of Proposition 3 We use the expression for $\frac{\partial t(q,n)}{\partial B}$ from above to write that:

$$\frac{\partial}{\partial s}\frac{\partial t}{\partial B} = \frac{(\delta N + D^C\omega)\omega\frac{\partial D^C}{\partial s} - D^C\omega\omega\frac{\partial D^C}{\partial s}}{(\delta N + D^C \omega)^2} \tag{25}$$
$$= \frac{\delta N \omega}{(\delta N + D^C\omega)^2}\frac{\partial D^C}{\partial s} \tag{26}$$

Thus, showing the first comparative static is equivalent to showing that $\frac{\partial D^C}{\partial s} > 0$. We compute:

$$\begin{aligned}
\frac{\partial D^U}{\partial s} &= q\frac{\partial D^{L,U}}{\partial s} + (1-q)\frac{\partial D^{H,U}}{\partial s} && (27)\\
&= -q\frac{D^{L,U}}{1-s} - q\delta D^{L,U}\frac{\partial t^{L,U}}{\partial s} - (1-q)\frac{D^{H,U}}{1-s} - (1-q)\delta D^{H,U}\frac{\partial t^{H,U}}{\partial s} && (28)\\
&= -\frac{D^U}{1-s} - \frac{\delta D^U}{1-\gamma}\frac{\partial \lambda}{\partial s} && (29)
\end{aligned}$$

and likewise:

$$\frac{\partial D^C}{\partial s} = \frac{D^C}{s} - \frac{(\delta+\omega)D^C}{1-\gamma}\frac{\partial \lambda}{\partial s} \tag{30}$$

Then we solve for λ_s by again taking derivatives of the constraint:

$$\begin{aligned}
& \frac{\partial D^U}{\partial s} + \frac{\partial D^C}{\partial s} = 0 && (31)\\
\Rightarrow\quad & -\frac{D^U}{1-s} + \frac{D^C}{s} = \frac{1}{1-\gamma}\frac{\partial \lambda}{\partial s}\left[\delta D^U + (\delta+\omega)D^C\right] && (32)\\
\Rightarrow\quad & \lambda_s = \frac{(1-\gamma)(D^U/s - D^U/(1-s))}{\delta D^U + (\delta+\omega)D^C} && (33)\\
\Rightarrow\quad & \lambda_s = \frac{(1-\gamma)(D^C - N)}{s(1-s)\left[\delta N + \omega D^C\right]} && (34)
\end{aligned}$$

Thus:

$$\begin{aligned}
\frac{\partial D^C}{\partial s} > 0 \quad &\Leftrightarrow\quad \frac{1}{s} > \frac{(\delta+\omega)}{1-\gamma}\frac{(1-\gamma)(D^C-N)}{s(1-s)(\delta N + \omega D^C)} && (35)\\
&\Leftrightarrow\quad 1-s > \frac{(\delta+\omega)(D^L-N)}{\delta N + \omega D^L} && (36)
\end{aligned}$$

Since the RHS is negative, this inequality is always true.

Sensitivity of tuition response to for-profit motive Here we show that

$$\frac{\partial}{\partial \gamma}\frac{\partial t}{\partial B} < 0 \Leftrightarrow \frac{D^{H,C}}{D^C} < \frac{\delta D^{H,U} + (\delta+\omega)D^{H,C}}{\delta D^U + (\delta+\omega)D^C} \tag{37}$$

We use the expression for $\frac{\partial t(q,n)}{\partial B}$ from above to write that:

$$\frac{\partial}{\partial \gamma}\frac{\partial t}{\partial B} = \frac{\delta N\omega}{(\delta N + D^C\omega)^2}\frac{\partial D^C}{\partial \gamma} \tag{38}$$

and thus that we want to show that: $\frac{\partial D^C}{\partial \gamma} < 0$. We compute the derivatives of the demand function with respect to γ as follows:

$$\begin{aligned}
\frac{\partial D^{q,U}}{\partial \gamma} &= -\delta D^{q,U}\left[-\frac{q+\lambda}{(1-\gamma)^2} + \frac{1}{1-\gamma}\frac{\partial \lambda}{\partial \gamma}\right] && (39)\\
\frac{\partial D^{q,C}}{\partial \gamma} &= -(\delta+\omega)D^{q,C}\left[-\frac{q+\lambda}{(1-\gamma)^2} + \frac{1}{1-\gamma}\frac{\partial \lambda}{\partial \gamma}\right] && (40)
\end{aligned}$$

We use these to solve for λ_γ:

$$\frac{\partial D^{H,U}}{\partial\gamma}+\frac{\partial D^{L,U}}{\partial\gamma}+\frac{\partial D^{H,C}}{\partial\gamma}+\frac{\partial D^{H,C}}{\partial\gamma}=0$$

$$\Rightarrow \quad \frac{\delta(D^{H,U}(q_H+\lambda)+D^{L,U}(q_L+\lambda))+(\delta+\omega)(D^{H,C}(q_H+\lambda)+D^{L,C}(q_L+\lambda))}{(1-\gamma)^2}=\frac{\partial\lambda}{\partial\gamma}\frac{1}{1-\gamma}\left[\delta D^U+(\delta+\omega)D^C\right]$$

$$\Rightarrow \quad \lambda_\gamma=\frac{\delta(D^{H,U}(q_H+\lambda)+D^{L,U}(q_L+\lambda))+(\delta+\omega)(D^{H,C}(q_H+\lambda)+D^{L,C}(q_L+\lambda))}{(1-\gamma)\left[\delta D^U+(\delta+\omega)D^C\right]}$$

$$\Rightarrow \quad \lambda_\gamma=\frac{\delta D^{H,U}+(\delta+\omega)D^{H,C}}{\delta D^U+(\delta+\omega)D^C}\frac{q_H+\lambda}{1-\lambda}+\frac{\delta D^{L,U}+(\delta+\omega)D^{L,C}}{\delta D^U+(\delta+\omega)D^C}\frac{q_L+\lambda}{1-\lambda} \tag{41}$$

Thus:

$$\frac{\partial D^C}{\partial\gamma}=-(\delta+\omega)\left[D^C\frac{1}{1-\gamma}\frac{\partial\lambda}{\partial\gamma}-D^{H,C}\frac{q_H+\lambda}{(1-\gamma)^2}-D^{L,C}\frac{q_L+\lambda}{(1-\gamma)^2}\right] \tag{42}$$

$$\frac{\partial D^C}{\partial\gamma}<0 \quad\Leftrightarrow\quad \lambda_\gamma>\frac{D^{H,C}}{D^C}\frac{q_H+\lambda}{1-\gamma}+\frac{D^{L,C}}{D^C}\frac{q_L+\lambda}{1-\gamma} \tag{43}$$

$$\Leftrightarrow\quad \frac{\delta D^{H,U}+(\delta+\omega)D^{H,C}}{\delta D^U+(\delta+\omega)D^C}>\frac{D^{H,C}}{D^C} \tag{44}$$

where the final implication follows from the fact that the left and right sides are both weighted sums of $\frac{q_H+\lambda}{1-\gamma}$ and $\frac{q_L+\lambda}{1-\gamma}$ where the weights sum to 1, and $q_H>q_L$.

B Overview of the postsecondary education industry

This Appendix provides basic facts about the postsecondary education industry. As discussed above, average undergraduate per student tuition nearly doubled between 2001 and 2012, from about $6,950 to more than $10,000 in 2012 dollars (Figure 1), corresponding to an average real rate increase of 3.5% per year.

These overall trends in college tuition mask significant variation within the postsecondary education sector. Tuition at postsecondary educational institutions varies widely depending on the type of degree the institution offers (four-year bachelor's degrees, two-year associate's degrees, or certificates generally requiring less than two years of full time study) and by the type of governance it operates under (for example, non-profit or for-profit).

In the 2011-2012 school year, there were 10.7 million undergraduate students enrolled at four-year institutions, and 7.5 million students enrolled at two-year institutions (see Figure 8). Four-year institutions also enrolled an additional 2.8 million graduate students, though we focus mainly on undergraduate loan amounts and tuition in this paper. Four-year institutions, which include public state universities (60% of enrollment in 2012), private non-profit research universities and liberal arts colleges (29%), and private for-profit institutions (11%), rely on a combination of revenue sources, from government appropriations to tuition revenue to other revenue (mostly private endowments and gifts). The two-year sector is almost entirely dominated by public two-year colleges, also known as community colleges, which enroll about 95% of all two-year students. Tuition at these colleges is low, averaging just $2,600 in 2012. Most of the revenue (70%) of these colleges instead comes from government sources.

Finally, in addition to the 20.4 million students enrolled at degree-granting institutions (two-year and four-year institutions) in 2012, another 572,000 were enrolled at Title IV "less-than-two-year" institutions. These institutions are mostly vocational schools in fields such as technology, business, cosmetology, hair styling, photography, and fashion. In contrast to the degree-granting institutions, the majority of these institutions are private for-profit institutions and tuition revenue makes up the majority of their funding.

The above numbers only cover Title IV institutions, but many for-profit institutions exist that are not Title IV-eligible.[22] Data on these institutions is hard to find since they are not tracked by the US Department of Education, but

[22] All public institutions are eligible for Title IV. Other institutions must meet certain qualifications such as being licensed, accredited from a Nationally Recognized Accrediting Agency (NRAA), and meeting standards of administrative

Cellini and Goldin (2014) construct a dataset using administrative data from five states, and show that, after controlling for observables, tuition at Title-IV-eligible for-profit institutions are 75% higher than comparable non-Title-IV-eligible for-profit institutions.

C Additional earnings calls transcripts

In this Section we provide additional passages taken from earnings calls of the Apollo Group discussing the changes in federal student aid maximums.

<**Q - Mark Marostica**>: My question first relates to Brian's comment on the national pricing strategy, and I was wondering if you can give us some more specifics around that and whether or not you are actually planning to lower prices as part of that.
<**A - Brian Mueller**>: It is something that we are considering. I have talked about it the last couple of conferences we've attended. We have a very unique opportunity in July. Loan limits go up for first and second level students, which is fairly long overdue. By the time we get to July I am estimating that upwards of 70% of all students who are studying at the University of Phoenix at the level one and level two at those levels will be at Axia College at Axia College tuition rates. So there will be some room for us to raise tuition there from maybe 265 to 295 and from 285 to maybe 310, without putting a burden on students from a standpoint of out-of-pocket expense. At the graduate level there is a lot of room. We are actually quite a bit under the competition in our graduate programs, and there is a lot of room from a Title IV standpoint so that, again, we wouldn't put a burden on students from an out-of-pocket expense.
Source: Apollo Education Group, 2006:Q4 Earnings Call, accessed from Bloomberg LP Transcripts.

<**Q - Mark Hughes**>: And then any early view on whether Axia, with the price increase there affecting start levels in May?
<**A - Brian Mueller**> Whether it's affecting start levels in May?
<**Q - Mark Hughes**>: Right. 10% increase in tuition. Is anybody balking at that, or trends steady?
<**A - Brian Mueller**>: No, thank you for asking that. No, because loan limits are raised on July 1, for level 1 and 2 students. And so students know as they go in if they're going to have enough title IV dollars to cover the cost of their tuition, so, no, it's not impacting new student starts.
Source: Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP Transcripts.

<**Q - Brandon Dobell**>: One final one. Maybe as you think about discounting, at least the philosophy around affordability, pricing, discounting across the different brands or different programs, maybe, Brian, if you could speak to, has there been any change in terms of how you guys think about that? Do you think that discounting generates the wrong type of student or the right type of student, or how flexible do you think it will be going forward in terms of how you think about affordability issues?
<**A - Brian Mueller**>: We're not changing our thinking about that. It's really clear what's going on in the country economically, with the middle class getting squeezed. People don't have disposable income to spend for private school education but they understand its impact on their long-term career so they're willing to borrow the money at really good rates from a Title IV standpoint. And so if you can build your operations to the point that you can be profitable and keep those tuition rates inside Title IV loan limits you're going to do positive things with regards to retention, which will offset maybe the 4 to 6% increases that we would have gotten in the past.
Source: Apollo Education Group, 2007:Q2 Earnings Call, accessed from Bloomberg LP Transcripts.

D Stock market event study analysis

Here we discuss stock market responses of publicly traded for-profit institutions to the three legislative changes discussed in Section 3. Table A1 reports event studies for abnormal returns over 3-day windows surrounding the passage of the three legislative changes to the HEA. Fourteen for-profit education companies were publicly traded around at least one of these legislative changes (and eight across all changes), including the Apollo Education Group among others. The

capacity and financial responsibility (e.g., default rates of graduates in excess of 25% for three consecutive years, or a one-year default rate in excess of 40%, are grounds for losing Title IV status).

cumulative abnormal returns are computed as each stock's excess return to the CRSP index returns, summed over the 3-day event window. We then calculate the (market cap) weighted and unweighted average of the cumulative abnormal returns of the eight publicly traded for-profit institutions to the index.

In the top panel of Table A1, we see that average 3-day cumulative abnormal returns around the 2006 re-authorization of HEA, which increased the subsidized loan limits for freshman and sophomores, were 3.64% and 2.9% under the value- and equally-weighted market benchmarks, respectively. The abnormal returns are statistically significant and economically large. As shown in the middle panel, three-day cumulative abnormal returns surrounding the 2007 legislative passage that increased Pell Grant amounts were 2.17% and 2.22%, respectively. Finally, we consider two separate event windows for the passing of the Ensuring Equal Access to Student Loans Act of 2008 which increased unsubsidized borrowing amounts.[23] Depending on the exact window used, abnormal returns on the for-profit institution portfolio ranged between 4.8% and 3.3%.

In sum, we find evidence that the passage of three pieces of legislation were associated with sizable abnormal stock market responses for the portfolio of publicly traded for-profit institutions. The nearly 10% abnormal return is consistent with the fact that students at for-profit institutions rely heavily on federal student aid to fund their education. In addition, anecdotal evidence also supports the view that changes in Title-IV programs boosted tuition at these institutions.

Table A1: **Stock Market Reactions to Changes in Federal Aid Policy** This table reports 3-day cumulative abnormal returns for a portfolio of 14 publicly traded for-profit universities surrounding dates of legislative passage to changes in Federal Aid Policy. Returns are computed in excess of the CRSP index on a value-weighted and equal-weighted basis.

Event	Date	Mkt Weights	Policy	Event Window	Mean Cum. Abnormal Ret.	Z score
Congress reauthorized the Higher Education Act	2/1/2006	v	Sub./Unsub. Loans	(-1,+1)	3.64%	(3.216)
		e	Sub./Unsub. Loans	(-1,+1)	2.90%	(2.545)
College Cost Reduction and Access Act Passes Congress	9/7/2007	v	Pell Grants	(-1,+1)	2.17%	(2.204)
		e	Pell Grants	(-1,+1)	2.22%	(2.242)
Ensuring Equal Access to Student Loans Act of 2008 is passed by the Senate	4/30/2008	v	Unsub. Loans	(-1,+1)	4.86%	(2.570)
		e	Unsub. Loans	(-1,+1)	4.80%	(2.480)
Ensuring Equal Access to Student Loans Act of 2008 is passed by Congress	5/1/2008	v	Unsub. Loans	(-1,+1)	3.30%	(1.752)
		e	Unsub. Loans	(-1,+1)	3.62%	(1.933)

E Data detail

This Appendix complements Section 4 in providing a more detailed data description. The data used in the empirical analysis throughout this paper comes from three sources: IPEDS, Title IV, and NPSAS. We provide institutional details on each. We then describe in detail the variables we constructed using the data from each of these sources.

Survey Data: The IPEDS survey covers seven areas: institutional characteristics, institutional prices, enrollment, student financial aid, degrees and certificates conferred, student retention and graduation rates, and institutional hu-

[23]On April 30, 2008 the Senate passed the Act, after already having received approval by the House. However, the Senate's approving vote included some changes that had to be subsequently ratified by the House. Thus, the bill essentially passed on April 30, 2008, but the changes made by the Senate were not voted on, and subsequently passed by the House, until May 1, 2008. For completeness, we estimate three-day abnormal returns around both event dates, though the two event window obviously overlap on one day.

man resources and finances. While IPEDS is the most comprehensive dataset on postsecondary education available, because it is based on surveys of administrators, it is not always sufficiently detailed or reliable for our purposes. For measures of federal aid at the institutional level, we found that the figures contained in the IPEDS "Student Financial Aid" survey did not meet our needs for a couple reasons. First, the survey restricts the universe to aid amounts for "full-time first-time degree-seeking undergraduates," which is not our student population of interest; second, in part because of this restriction, the survey has been labeled as the most burdensome of surveys (Government Accountability Office (2010)); and third, until recently, the survey did not distinguish between federal loans and other loans, and still does not distinguish between subsidized and unsubsidized loans, which makes our identification more difficult.

Title IV data serve as our primary data source for measuring federal loans and pell grants at the institution level. While we considered also using IPEDS to obtain these measures, we ultimately found a number of reasons to look to the Title IV data. One of the reasons is that the IPEDS measures of financial aid are contained in the "Student Financial Aid" survey, which is considered by most educational administrators to be the most burdensome of the IPEDS surveys (Government Accountability Office (2010)). This is likely because it requires administrators to estimate the total amount of aid and number of recipients within a specific IPEDS-defined universe of students, "full-time first-time degree-seeking undergraduates." Restricting to this universe may be difficult for some institutions depending on what data sources they pull from to complete the IPEDS surveys. Thus, these data are less reliable than those obtained from the less-burdensome collection of published tuition levels and enrollment numbers. Second, this universe is not necessarily representative of the entire undergraduate body. Third, until recently, IPEDS did not distinguish between federal loans and other loans, and still does not distinguish between subsidized and unsubsidized loans, which makes our identification more difficult. We describe the benefits of the Title IV data relative to the IPEDS data in Section 4 in the main body of the text.

Sample: Our sample begins in the 2000-2001 school year, the first year that the tuition sticker price survey from IPEDS more or less takes the current form. We end our sample in 2011-2012, since in 2012-2013, changes to graduate financial aid occur that may interfere with our identification. IPEDS and NPSAS data are reported at institution level (UNITID), while Title IV is reported at the OPEID level. This is because there may be multiple UNITIDs associated to one OPEID, as branches (UNITID) of the same institution are sometimes surveyed separately. Our regressions are done at the OPEID level, where when we are using averages of variables in IPEDS, we take enrollment-weighted averages of the UNITIDs.

Sticker-Price Tuition: Our main dependent variable is yearly changes in the sticker-price tuition at the institutional level. This data comes from the IPEDS Student Charges survey. For full academic-year programs, we use the sum of the out-of-state average tuition for full-time undergraduates and the out-of-state required fees for full-time undergraduates. For other programs, we use the published tuition and fees for the entire program. For public universitites we use out-of-state tuition rather than average tuition to abstract from variation driven by changing fractions of in-state versus out-of-state students. We generally find that the in-state and out-of-state differences are highly correlated.

Enrollment: Enrollment can be measured both as headcount and full-time equivalent students. In general, we use an IPEDS formula to calculate a full-time-equivalent (FTE) enrollment measure. In certain cases though, we use total headcounts from the IPEDS enrollment survey, which are available by student level and attendance status.

Federal Loan and Grant Usage: For federal loan and grant totals, we rely on Title IV administrative data rather than the student financial aid survey from IPEDS, which appears to be somewhat unreliable as it is survey based. Title IV data contains the number of recipients, and total dollar amount of loans originated or grants disbursed for each institution and each of subsidized loans, unsubsidized loans, and Pell Grants. We only consider undergraduate policy changes and tuition in this paper, so we would want these amounts to be for undergraduates only. However, Title IV data does not break out undergraduate and graduate loans separately until 2011. Pell Grants are only available to undergraduates, so are not affected. Since imputation of an undergraduate measure requires making several assumptions, our preferred measure of loan and grant usage at an institution is just the total dollar amount scaled by the FTE count of the university. We also report results for robustness when we scale the total dollar amount by the total enrollment count. Finally, also for robustness, we make an attempt to impute an undergraduate measure as follows: Since the maximum subsidized loan amount changes only for undergraduates in our sample, we assume a constant average graduate loan amount over time, $\bar{g}_i$ conditional on borrowing. In addition, we assume that the fraction of all subsidized loan borrowers at an institution who are graduate students also does not change, γ_i. To calculate $\bar{g}_i$ and γ_i, we take the averages of the 2011 and 2012 values.[24] For prior years, given the total subsidized loan amount S_{it}, we calculate the undergraduate dollar amount borrowed as: $S_{it} - \gamma_i \bar{g}_i$. We then scale this measure by total undergraduate enrollment.

Exposures: We calculate exposures using confidential NPSAS data as described in Section 4.3.

Net Tuition and Institutional Grants: Our institutional grant data comes from the IPEDS Finance Survey, which records as an expenditure item total grant dollars spent on scholarships and fellowships. We scale this measure by the FTE enrollment. We compute net tuition by subtracting institutional grants per FTE from sticker price.

Financing Controls: We follow the Delta Cost Project data in separating revenue data into a few main parts. The first is net tuition revenue, as described above. The next is federal funding, excluding Pell Grants. The third is state (and local) funding through appropriations and contracts. The fourth is private funding (from donations, or endowment investment income), and the fifth is revenue from auxiliary operations (e.g. hospitals, dormitories). We use changes in these amounts, scaled by FTE enrollment, as controls in our regressions.

Other Controls: Average EFC comes from NPSAS data, and the admission rate comes from IPEDS.

F Additional robustness tests

Using 2008 NPSAS exposures: In the baseline specification we measure institution exposures using the 2004 NPSAS wave, the closest available wave that still predates the changes in loan (and most of the grant) maximums. Despite the results in Table 3, one may worry about the time gap between when the exposures are computed and when the policy changes take place. In Table A2 we re-estimate the baseline specification using exposures computed from 2008 NPSAS for robustness. Aid sensitivities to changes in the institution-specific program aid maximums as of 2008 maximums (columns 1-3) are very similar to the 2004 ones, with the exception of the subsidized loan sensitivity response to the subsidized loan maximums, which increases to 1.25 from .7 in Table 3. Subsidized loan maximums are increased in 2008, so that the 2008 subsidized loan exposure is measured at the post-policy maximum amounts. To the extent that not all students fully expanded their borrowing (as suggested by comparing the 2004-08 subsidized exposures in Table 2 and the loading in Table 3), the sensitivity of 2008 to 2004 subsidized exposures drops, resulting in a higher point estimate in column 3. Sticker tuition displays a very similar sensitivity to the institution-level change in program maximums (compare columns 4 in Tables 3 and A2), although the point estimate on Pell Grants is less precisely estimated (t-stat = 1.66). In Table A3, we repeat the IV estimates of Table ?? using exposures computed as of 2008 NPSAS and obtain very similar results, except for a lower sensitivity of sticker tuition to subsidized loans owing to the overstated pre-policy exposure discussed above.

Dependent variables in logarithms: Because changes in federal aid policies affected dollar levels, rather than percentage changes, of the program maximums, the dependent variables in our baseline specification are expressed in dollar changes. In Table A4 we re-estimate the specification with the dependent variable expressed in logarithmic changes. While this specification does not directly match the policy change, it can be informative about the magnitude of percentage effects of the changes in program caps. Starting with the percentage change response of aid levels, Pell amounts (column 1) now load with an incorrect (negative) sign on changes in Pell caps.[25] Subsidized and unsubsidized loans (columns 2 and 3) load positively on changes in their respective caps and negatively on the Pell Grant caps suggesting substitution from loans to grants, as in the baseline specification in dollar changes. Finally, in terms of percentage changes in tuition, a $100 increase in the program caps resulted in .4%, .2% and .1% (statistically significant) increases, respectively, for Pell Grants, subsidized, and unsubsidized loans.

[24]We drop institutions from our sample where the 2011 and 2012 values differ significantly.

[25]This may owe to the percentage-change specification along with the fact that, because of the program design, Pell Grant exposures include all recipients receiving a positive, rather than only those at the program maximums as it is the case for subsidized and unsubsidized loans.

Table A2: **Baseline regression specification using 2008 NPSAS exposures** This table replicates Table 3 using NPSAS aid exposures as of 2008 as opposed to 2004 ones. See notes to Table 3 for more details. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

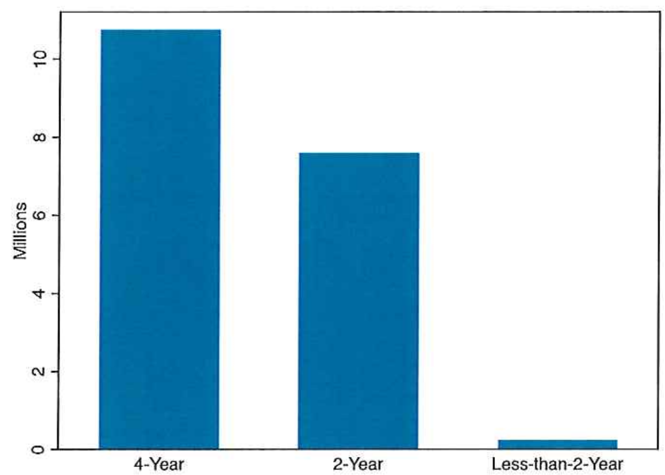
	(1) ΔSubLoans$_{it}$	(2) ΔUnsubLoans$_{it}$	(3) ΔPellGrants$_{it}$	(4) ΔStickerTuition$_{it}$	(5) ΔStickerTuition$_{it}$
SubLoanExp08$_i$ × ΔSLCap$_t$	1.223***	0.106	0.108	0.609***	
	[0.11]	[0.16]	[0.08]	[0.23]	
UnsubLoanExp08$_i$ × ΔUSLCap$_t$	0.030	0.650***	-0.057***	0.233***	
	[0.02]	[0.04]	[0.01]	[0.04]	
PellGrantExp08$_i$ × ΔPGCap$_t$	-0.346***	-0.466***	0.997***	0.283	
	[0.08]	[0.15]	[0.09]	[0.17]	
ΔSubLoans$_{it}$					0.891**
					[0.35]
ΔUnsubLoans$_{it}$					0.243**
					[0.10]
ΔPellGrants$_{it}$					0.527***
					[0.18]
Inst&Year FE?	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.08	0.23	0.48	0.39	-0.45
N Obs	13610	13540	14000	14500	9320
N Inst	1340	1350	1410	1420	970

Table A3: **IV regression specification using 2008 NPSAS exposures** This table replicates Table ?? using NPSAS aid exposures as of 2008 as opposed to 2004 ones. See notes to Table ?? for more details. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

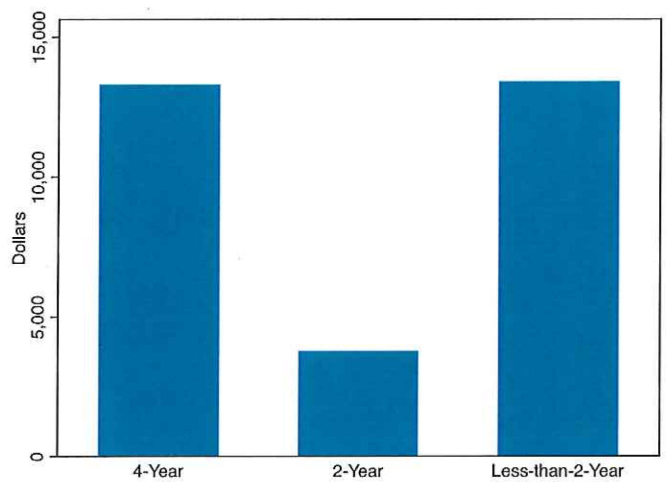
ΔStickerTuition$_{it}$	(1)	(2)
ΔPellGrants$_{it}$	0.270*	0.559**
	[0.16]	[0.18]
ΔSubLoans$_{it}$		0.502**
		[0.22]
ΔUnsubLoans$_{it}$		0.352**
		[0.07]
Inst&Year FE?	Yes	Yes
N Obs	13110	13110
N Inst	1340	1340

Figure 8: **Enrollments, Sticker Tuition and Revenue by Program Type** These figures plot total enrollment, average sticker price, and average revenues per student for institutions, depending on the type of program offered in the 2011-2012 school year. Source: IPEDS.

(a) **Total undergraduate enrollment by institution program type (millions)**



(b) **Average sticker price by institution program type**



(c) **Average per-student revenues by institution program type**

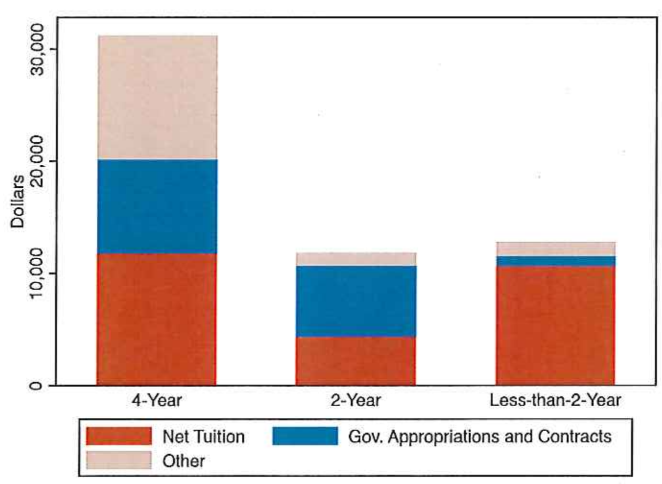


Table A4: **Baseline regression specification with dependent variables in logarithmic changes** This table replicates Table 3, but uses percentage changes in the dependent variables rather than changes in absolute terms. See notes to Table 3 for more details. Sample sizes are rounded to the nearest 10 in compliance with NPSAS nondisclosure policies. Standard errors clustered at the institution level reported in brackets. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Δ logPellGrants$_{it}$	(2) Δ logSubLoans$_{it}$	(3) Δ logUnsubLoans$_{it}$	(4) Δ logStickerTuition$_{it}$
SubLoanExp$_i$ × ΔSLCap$_t$	0.009*	0.009*	0.000	0.002*
	[0.00]	[0.01]	[0.01]	[0.00]
UnsubLoanExp$_i$ × ΔUSLCap$_t$	-0.003***	0.001	0.014***	0.001***
	[0.00]	[0.00]	[0.00]	[0.00]
PellGrantExp$_i$ × ΔPGCap$_t$	-0.016***	-0.017***	-0.026***	0.004**
	[0.00]	[0.00]	[0.01]	[0.00]
Inst&Year FE?	Yes	Yes	Yes	Yes
Adj R^2	0.49	0.13	0.19	0.04
N Obs	10040	9740	9730	10480
N Inst	1040	990	990	1060